Opening Address
ON THE
USE OF FORCEPS AND ITS ALTERNATIVES IN LINGERING LABOUR,

Delivered at the Debate on the subject at the Obstetrical Society of London, May 7th, 1879.

BY ROBERT BARNES, M.D., F.R.C.P.

THE theme which I have been invited by the Council to bring before the Society for discussion is the following:—"On the use of the Forceps and its Alternatives in lingering Labour."

It was obviously necessary to confine the discussion within certain definite boundaries, not alone in order to avoid irrelevant and wasteful debate, but also as the most essential condition for arriving at clear and useful conclusions.

And when we refer to "conclusions" as the result of our discussions we shall not aim at the framing of conclusions which shall be final or absolute, or have the force of law. We could not engage in a more vain and injurious undertaking. Science is in her very essence progressive. Freedom from arbitrary dogmas is the condition of her life. It is that freedom which impels or invites every one of us to try this, like every other scientific question, by the patient and candid observation of nature, acknowledging no mere tradition or authority. The true function of a Society is to gather together and then to diffuse knowledge; to encourage independent inquiry; to survey from time to time, by the light of mutual reflexion, the positions attained; and thus to seek sound guidance in the application of our knowledge to our practical duties.

Ever so superficial a retrospect over the history of the forceps will show us that more than once the use of this instrument has been defined in the most absolute and inflexible terms. And although arbitrary laws may have met with wide obedience, many men have always refused to be fettered by them. Some of these rebels may have carried their contempt against authority too far; they may have run into contrary, even licentious extremes. But they are at any rate entitled to this praise, that they have kept alive the spirit of independence, have compelled us to think over again, to doubt whether the laws we had been summoned to submit to were the expression of absolute wisdom.

At this moment practice differs widely. And this difference is not so much the result of the different teaching of the schools, as it is of local and individual accidents. Side by side we see men using the forceps once in every nine or ten cases or oftener still, and others using it perhaps once in a hundred cases or even less; and this under conditions that offer no explanation of the difference in practice. Looking at the actual practice of the day, we may thus see repeated and continued the lessons of history. From the epoch of the invention of the forceps nearly, the same diversity of practice has existed. Indeed, during the early days of the forceps—speaking of this country, the birthplace of the forceps—it seems probable that the forceps was more commonly employed than it was thirty or forty years ago. Now and then the use has become so free as to take the character of abuse; and then an outcry has arisen, followed by reaction and restriction. Thus, preceding and during the time of William Hunter, Osborne, and Denman, instrumental interference was frequently had recourse to unnecessarily, and unskilfully we may assume, since these illustrious men thought it right to exert all their authority in discouraging the use of instruments, and in inculcating blind faith in Nature as the better way. So earnest was their zeal, so commanding was their authority, that men were driven back into the opposite extreme of supine inaction. Rules were laid down, and widely obeyed, which too frequently allowed parturient women to drift into danger, injury, and death. Thirty years ago, even, the law of Denman was taught that enjoined us to wait until the child's head had rested for six hours on the perineum before resorting to the forceps; and this policy of procrastination was scarcely mitigated by the associated rule that "no case was to be esteemed eligible for the forceps unless the ear of the child could be distinctly felt." Many of us may remember this sort of teaching. Its influence may have been beneficial in repressing reckless and unskilful operators. But, as might be expected, the neglect of the forceps has entailed the very dangers for which it was denounced. The casuist may balance the degrees of culpability of the man who, seeking to help a woman, injures or destroys her, and of him who, trusting alone to hope, lets a woman sink into perilous exhaustion and death. The result to the victim is the same. And we, weighing the men in the scales of science, may see more hope for humanity in the enterprise of the man who acts than in the blind helplessness of the man who does nothing. The first may improve—he may acquire judgment and skill; for the man who is tied hand and mind to a policy of waiting there is no hope.

So far is the question of the application of the forceps from being settled, that when we come to examine it critically we find ourselves brought face to face with some of the most difficult problems in obstetric practice.

The limitation of our inquiry to "the use of the forceps or its alternatives in lingering labour," implies the exclusion from our consideration:

1. Of those cases in which acceleration of labour is indicated by convulsions, by hæmorrhage, by other complications, as syncope, dyspnœa, apoplexy.

2. Of those cases in which the child is in danger from prolapse of the umbilical cord or other causes independent of protracted labour.

In studying the question before us, we are naturally drawn to institute a comparison between the results to mother and child, as to injury and death, under the two opposite practices of free resort to the forceps, or to its alternatives; these alternatives being understood to comprise:—(1) simple inaction, or expectancy; (2) ergot or other oxytocics; (3) the fillet or lever; (4) compression of the uterus, or other manœuvres, not instrumental.

This, the statistical method, which seems to promise so much, is not free from objection. It is in the first place not easy to obtain facts that are fairly comparable. And even could this postulate be secured, it would not follow that we could decide for or against the free use of the forceps because the injuries and deaths of mother and infant were more frequent under resort to the forceps or to its alternatives.

This brings us to the definition of the state which demands or justifies interference. How shall we define this state?

Several reasons concur in leading me to quote the rules of Collins, the younger Ramsbotham, and George Johnston.

Collins gives the following as the conditions for using the forceps:—"Generally speaking, so long as the pulse remains good, the bowels and bladder act well, the soft parts remain free from severe pressure, and uterine action continues so as to cause the presenting part to descend ever so slowly, the patient having no pain in the abdomen on pressure, or local distress, the child at the same time being alive, as indicated by the stethoscope, I am satisfied no attempt should be made to deliver with instruments, and that he who does so wantonly exposes both mother and child to danger..... The necessity alone of freeing our patient from impending or present danger should induce us to resort to instruments."

Ramsbotham says: "If pains are subsiding gradually or have disappeared, strength failing, spirits sinking, countenance anxious, pulse 120 or more, tongue coated, dry, brown, or raspy, two or three rigors, tenderness on pressing abdomen, green discharge, preternatural soreness of vulva, heat and swelling of vagina, head locked, vomiting, hurried breathing, delirium, or coldness of extremities, *then delivery must of necessity be effected*, and we should be acting most injudiciously to allow the case to proceed until the four last-named symptoms appear without relief being offered..... But so long as the uterus is contracting with energy, the strength and spirits good, the countenance natural and cheerful, pulse under 100, tongue and mouth moist and clean, no vomiting or rigors or heat, swelling or tenderness of parts, so long as the head retreats in the absence and advances in the presence of pain, provided there be any progress in the labour from hour to hour, so long there can be no necessity for instrumental aid."

George Johnston states the case as follows (1872):—"We have come to the conclusion, and our established rule is, that so long as nature is able to effect its purpose without prejudice to the constitution of the patient, danger to the

soft parts, or the life of the child, we are in duty bound to allow the course of labour to proceed. But as soon as we find the natural efforts are beginning to fail, and after having tried the milder means for relaxing the parts or stimulating the uterus to increased action, and the desired effects not being produced, we consider we are justified in adopting prompter measures, and by our timely assistance relieve the sufferer from her distress and danger and her offspring from an imminent death."

Strictly analysed and compared, it will be found that the rules for interference as laid down by Collins, Ramsbotham, and George Johnston, respresenting as they do three distinct epochs and schools of practice, are all but identical.

How men recognising the same principles could diverge so widely in practice is a curious study. It constitutes the very kernel of our discussion.

Collins, in the Rotunda Hospital, in the years 1826 to 1833, used the forceps or lever once in 607 cases. Johnston, in the same hospital, in the years 1868 to 1875, used the forceps once in every 10 or 11 labours.

Somewhat intermediate in time between Collins and George Johnston, and practising under conditions somewhat different, stands Francis Ramsbotham, who with David Davis may fairly be said to have held the foremost position during their time in this metropolis for judgment, experience, and skill. Ramsbotham applied the forceps, in the work of the Royal Maternity Charity (1828 to 1850), once in 671 labours. There is no doubt he would have used the forceps more frequently had the cases all been under his control from first to last, as is the case in a lying-in hospital. But he could only act when called for by a midwife. Thus the midwife had exhausted the alternatives; and we may take the residuum in which the forceps was applied as representing practically the proportion of cases in which resort to the forceps was not a matter of election, but of necessity. Now, if we compare Ramsbotham's experience with Collins's, we find both using the forceps in public practice—the one in a hospital, the other in domiciles—at nearly the same rate of frequency. This lends strength to the conjecture that, other alternatives being duly tried, the proportion of cases that imperatively demand the forceps does not exceed one in five hundred. If this *primâ facie* case be supported by evidence that the damage and mortality endured by mothers and children compare favourably with the results of more active practice—say with Dr. Johnston's,—then we may be led to conclude that in very many cases Dr. Johnston resorted to the forceps when there was *no necessity* for the operation—that is to say, when the alternatives, as waiting or ergot, would have effected delivery with equal safety.

This does not carry the condemnation of Dr. Johnston's practice, for the question would still remain, after the establishment of the fact that the ultimate results to mother and child were similar under the practice of Collins, Ramsbotham, and Johnston, whether the forceps would not have been better in many of Collins's and Ramsbotham's cases than the alternatives which were trusted to. For example, there can be little doubt that in many cases under Collins and Ramsbotham waiting was carried to the verge of danger, whilst in Johnston's cases the dangers, immediate and remote, of waiting were averted, and the individual and aggregate suffering in actual pain was immensely curtailed. We must not forget that the forceps is not simply an operation of necessity, but that it is pre-eminently an operation of election.

Passing from public practice to private practice, we find in the first volume of our Transactions the records of two men of the highest stamp—the one, Robert Dunn, who practised in London; the other, W. H. Bailey, who practised in Thetford—that is, in a rural district. It may safely be assumed that both these men conducted their practices conscientiously and skilfully, and no one will doubt that they have recorded their experience faithfully. During twenty years ending with 1850 Dunn used the forceps twenty times, or once in 190 cases. He "often used ergot." He had ten cases of craniotomy. He says: "Not in one single instance has tedious or difficult labour proved fatal to mother or child." But two deaths after craniotomy must be considered to qualify this statement.

Bailey, during the fifty years ending in 1859, used the forceps, it is presumed, very rarely. He does not specify how often. He had only two cases of embryotomy.

Comparing Dunn and Bailey, we see, as might be expected, a marked contrast between the obstetric histories of a town and of a rural population. Dunn had a far larger proportion of difficult cases. Dunn's embryotomy cases were 1 in 400, whilst Bailey's were only 1 in 3200. Dunn's maternal deaths were 1 in 150 nearly, and Bailey's 1 in 430 only. It is a legitimate conjecture that Bailey and Dunn worked on very similar rules. The difference in their practice and in their results is almost certainly due to the difference in the subjects they had to deal with. Another caution in the use of statistics.

In discussing our question, we are met by the necessity of drawing a line between what McClintock called the "high" and the "low" forceps operations. The low operation takes simply those cases in which the head has entered the pelvic cavity; the high operation applies to those cases in which the head is seized at or above the brim. The distinction is imperative. The low operation may be accomplished by the short or single curved forceps. It is usually a very simple proceeding. The presence of the head in the pelvis implies, generally at least, that there is no obstacle from disproportion to be overcome, that the resistance of the cervix uteri and vagina has been overcome, and that there remains little or nothing beyond the resistance of the vulva. We merely want a little *vis à tergo* to complete expulsion. In this conjuncture some will prefer ergot, some will prefer forceps. My own decision has long been in favour of the forceps. I hesitate to affirm that the use of ergot is wrong. But the following reasons for preferring the forceps are to my mind sufficient:—1. Since the publication of Hardy and McClintock's report of their researches at the Rotunda, which showed that unless the child were born within a short time after the action of ergot it was very likely to be born dead, I have turned my attention to this point, and have seen abundant confirmation of the accuracy of their observations. 2. The action of ergot upon the uterus is uncertain. You want a certain amount of uterine action. You give ergot, and the desired result may ensue. But it may not. You may have too much, or too little. In the first event you have evoked a Frankenstein whose brute violence you can neither subdue nor regulate. The force which should be delicately adjusted to the resistance is in excess. And then you have to fear the very dangers of lingering labour—rupture of the uterus, other injury to the soft parts, and the death of the child—which you called in the ergot to avert. I prefer to enlist an ally which will do exactly what I want, and no more. 3. In a large proportion of cases of arrest of the head in the pelvis, the arrest is due to malposition of the head—for example, to occipito-posterior positions. The occiput turns up under the sacral promontory, and then the driving force propagated along the child's spine tells in a false direction, tending to roll the head more over on its transverse axis. It tends, in short, to revolve in extension around the promontory as a centre, instead of round the symphysis pubis. The driving force is wasted. Fortunately it often ceases or moderates. It is utterly unscientific, even dangerous, to goad it by ergot. The forceps is the true and effective help. It not only supplies the wanting force, but gives that force its proper direction, under conditions in the highest degree adapted to secure the wellbeing of mother and child.

I have now set forth what appears to me to be the first point of discussion—namely, the choice of the forceps or of its alternatives, when the head is arrested in the pelvis.

I apprehend that there is not much room for disagreement upon this point. It is practically solved. The low operation is very generally in use. And provided reasonable discretion dictate the line of acting, and fair skill be used with a good instrument, this operation will often save the mother much present suffering, will in many cases lessen or remove the risk of injury to the soft parts, lessen the risk of septicæmic fever, and promote the safety of the child. It is very easy indeed to apply the forceps in cases of this category unnecessarily. But, on the whole, it may be questioned whether, at least in dealing with women of weak fibre, nursed in luxury, more harm may not ensue from habitual inaction than from over-readiness to operate.

In discussing the uses of the forceps, we are naturally led to inquire what its powers are—what it can do?

The forces of the forceps are:—(1) tractile; (2) leverage; (3) compression or moulding of the head.

The short forceps and the long forceps differ widely in the degrees in which they enjoy these forces.

The short forceps has but moderate tractile power, a limited leverage power, and scarcely any appreciable compressive power, apart from what it derives from the external compression of the soft parts upon its blades.

But the long forceps possesses all three powers in a high degree. 1. It admits of tractile power greater than that of the natural expulsive power. 2. Its leverage power is greater, and may be often effective at a point beyond reach of the short forceps. 3. Its compressive power may be almost indefinitely increased, according to the construction of the instrument.

Now, since all these forces, or equivalents of them, are exerted by Nature, it is but logical to employ them in cases where Nature fails. In a well-devised and executed forceps-operation something is gained from every one of these forces. The best forceps will be that which enables the operator to utilise all these forces in due harmony. Thus, excess of traction is avoided by the aid of judicious leverage and compression. Excess of compression is avoided by judicious traction and leverage. Excess of traction and of compression—involving, the one, risk to the mother, the other, risk to the child—both admit of important mitigation by external compression and pushing, imitating the driving force of Nature. And thus the highest degree of safety is secured to both mother and child. To throw away these powers possessed in the highest degree by the long forceps is to abdicate a vital part of one of the most beneficent powers that science has ever put into our hands.

I have only a word of caution about the use of leverage in the high operation. It must be applied with care and moderation. Its advantages are not so manifest as in the low operation. But those who declaim against leverage absolutely must be reminded that to avoid it altogether is a physical impossibility. The great difficulty in practice is to find the equilibrium between the powers of the forceps and the resistance to be overcome.

We now come, then, to the question of the *high operation*, the great point upon which the contest turns. Here problems comparatively unknown in connexion with the low operation come into consideration. In the low operation we are mostly concerned with the second stage of labour, when the expulsion of the child is partly accomplished. When the high operation is in question we have commonly to act during the first stage of labour, when the child has made little or no progress, and therefore when in many cases the cervix uteri is imperfectly expanded. The head has to be sought and seized at or above the pelvic brim. This of course demands a long and double-curved forceps. There is a greater complication of factors; far more judgment and skill are required in deciding upon and in carrying out the operation. *What are the conditions which demand or justify the high operation?* I will first cite the plea of Johnston: "I became," says Johnston, "an advocate for the more frequent use of the forceps . . . . from having witnessed on many occasions the direful results arising from allowing the labour to continue for hours unrelieved. The painful effects of such a system became evident in numerous instances of inflammation and sloughing of the vagina, ruptured uterus, and other accidents, too often ending in fatal consequences." He especially dwells upon the injurious effects produced by long pressure of the head on the soft parts in cases where the liquor amnii has escaped at the commencement of labour, and the danger that arises from the head pressing on the expanded cervix uteri before the os is fully dilated. He asks: "Why should we permit a woman to undergo hours of torture? . . . . Why should she be allowed to waste her strength and incur the risks consequent upon the long pressure of the head upon the soft parts, the tendency to inflammation and sloughing of the vagina, or the danger of rupture of the uterus, not to speak of *the poisonous miasm* that emanates from an inflammatory state of the passages, the result of tedious labour, and which is one of the fertile causes of puerperal fever . . . . attributed by some to the influence of being confined in a large maternity?"

All this argument applies to the low operation; but, tested by Johnston's practice, it applies chiefly to the high operation.

Johnston is brought into direct collision with those authorities who teach that during the first stage of labour little mischief from pressure on the soft parts (we of course exclude cases of disproportion), and little other mischief, is to be apprehended during the first stage of labour; in short, that the term "lingering labour" has no practical significance until we arrive at the second stage. If we accept this doctrine the question is solved; our discussion is at an end; Dr. Johnston and those who advocate the high operation are out of court, or nearly so. But is this sentence just? Must we go back to the narrow dicta of William Hunter, Osborne, and Denman? Must we declare that Smellie, Perfect, David Davis, Francis Rambotham, to say nothing of our predecessors and contemporaries in Europe and America, who so frequently resort to the high operation, are all wrong? We should surely repudiate such a sentence, and none the less because many of us would be included in the same condemnation.

It may be at once admitted that there are cases of lingering labour during the first stage—that is, before the cervix uteri is fully expanded, and before the child's head has entered the pelvis.

One order of such cases may be quickly dismissed. Those in which the uterus is more or less paralysed or disordered in its action by excess of liquor amnii, by the presence of twins or too large a child, or by extreme rigidity of the cervix uteri. The first difficulty is often got over by simple puncture of the membranes. But not seldom the evacuation is not complete enough; the head drops upon the unexpanded os uteri, and blocks the way of escape like a ball-valve, and labour is suspended, "lingering." In such cases I have often obtained quick release by introducing one blade of the forceps. This lifts the head off the cervix, forms a channel for the flow of the ponded-up waters, and calls out the reserved diastaltic function. If the uterus and system have been so far weakened as not to respond to this tentative measure, the second blade is introduced, and the proper action of the forceps is invoked.

Secondly, labour also may become lingering in the first stage because the *child is dead*. When the child has been dead some time, it has lost its resiliency, and driving force acting upon it tends to compress it into a ball, instead of propelling it onwards. Craniotomy would be the proper alternative here. But until we know the child is dead, the forceps is preferable. And *monsters* commonly cause lingering labour for analogous reasons.

Then we come to a third order of cases, in which the liquor amnii has sufficiently escaped, and in which the head rests upon the brim, or is only partially engaged within the brim, and the cervix uteri is imperfectly expanded. We may here call to mind the general fact that the full dilatation of the cervix is effected by the eccentric pressure of the bag of membranes and of the child's head, and that consequently we do not expect to find full dilatation until the head can engage in the pelvic cavity. It is true that in some cases of very protracted labour, the uterus, shortening itself under long-continued efforts, may open, whilst the head is delayed above the brim. But the fact remains generally true that so long as the head is above the brim the cervix is only imperfectly expanded. Hence it follows that to apply the forceps to the head above the brim the blades must generally be passed through an imperfectly dilated cervix. We must recognise this fact because it is a direct infringement of the old canon never to use the forceps until the os is fully expanded. We see in this fact also one broad distinction between the low and the high operation.

What are the cases, the liquor amnii having escaped, the head resting on the pelvic brim, and the cervix imperfectly expanded, which call for the forceps or its alternatives? We may clear the ground by saying that the first alternative to try is patience and reasonable time. Ergot is open to even graver objection in this case than when the head is in the pelvis. Without condemning the practice of those who do use it, I may be permitted to repeat that I myself would not use it. Delay at the brim may be due to minor degrees of disproportion, and this is not always easy to diagnose. Ergot, therefore, may be injurious as well as uncertain.

There is another order of cases in which the head is delayed at the brim which involves the conditions of lingering labour—I mean cases of overhanging belly and uterus, in which the abdominal muscles fail to keep the long axes of the uterus and child in due correlation with the axis of the pelvis. Here the forceps is especially useful, even though the cervix be not fully dilated. The alternative is turning. Both are facilitated by delivery in the dorsal posture and by compression of the abdomen.

We may now apply the test, What is lingering labour? For it will scarcely be denied that if the signs of lingering labour are manifested whilst the head is above the brim, we are as much called upon to interfere as when the same signs are manifested, the head being in the pelvic cavity. And unquestionably these signs may occur. But they will occur much more rarely than at a later period—that is, during the

second stage of labour. To state the proposition is almost to prove it. Fatigue, exhaustion, is a question of time. It is not necessary then to insist that a patient's strength may take her easily through the first stage, and yet be inadequate to carry her through the second. Just as a pedestrian may do his first ten miles without distress, and yet break down during the next lap. It may be true that the first stage presents the greater difficulties, but then these are encountered by fresher powers.

Let us take it then as granted that the high operation is sometimes called for. But experience, as recorded by Collins, Ramsbotham, and a host of others, compels us to affirm that the *necessity for this operation* cannot be frequent. The frequent resort to the operation, then, must rest for its chief justification upon its *utility*, and in utility is included *safety*. Apart from the cases of necessity, when is the operation useful? And what are its dangers? I mean those pertaining to the operation itself.

I will first take the question of danger, assuming, of course, reasonable skill. The forceps has to be introduced through an imperfectly expanded cervix. Dr. Johnston admits danger. He is careful to say, "I must here caution the practitioner that this operation is not without danger in unskilful hands, by whom it should never be attempted." I am disposed to go further, and say that this operation is not without danger even in skilful hands. The width of the fenestra of the forceps is about two inches. When both blades are applied to the head we have a globe measuring generally more than four inches in diameter to bring through the os uteri. But the os uteri may be three inches or less in diameter. We must therefore apply the blades high above the os uteri. This is generally not very difficult. I have often done it. The difficulty comes later. You begin extraction. If the os is easily dilatable, and sometimes it is so, it is dilated after a process closely resembling that of nature. The head-globe is made to open it. But it frequently happens that the os is not so easily dilatable. The natural order and process of labour has been disturbed, partly it may be by the difficulty which made it lingering, and partly by the interference of the operator. This disturbance is very apt to entail, if not spasmodic rigidity, at any rate a passive unyieldingness of the cervix. Thus, what happens if you put on traction? The whole uterus and child are dragged down together some way, and sometimes very far towards the pelvic outlet. The blades of the forceps divergent with the head between them, will not come through the narrower os. Traction under these conditions must often be continued for a long time—an hour or more—before the head will come through. And during all this time the pressure upon the inner surface of the lower segment of the uterus, and upon the thin edge of the os uteri is very great, sometimes more than it will bear with impunity. Some amount of bruising is inevitable, more perhaps than it would have undergone if left to nature, and not seldom, as I know, the os uteri is torn. And this also I know that during this artificial dilatation of the cervix, the suffering of the woman is greater than when the dilatation is effected under the natural forces. Then again the disturbance of the natural process of parturition, the due sequence of its acts being perverted, entails after-trouble. I am sure that the disposition to retention of the placenta and hæmorrhage is increased. Now these are precisely the effects of lingering labour which we urge as our excuse for operating. What have we gained?

This objection applies with peculiar force to primiparæ. The cases must be extremely rare in which there is any danger from lingering labour in the first stage, apart of course from obstruction due to disproportion or malposition. Yet we find Dr. Johnston applying the forceps to 123 primiparæ, before the os uteri was fully dilated; in 103 there was "early rupture of the membranes." Of these, in 64 cases the head was above or in the brim; 9 of the women died, "3 from convulsions; 2 of gastro-enteritis; 1, sloughing of vagina; 1, peritonitis; 1, uterine diphtheritis; 1, distress of mind. In none of these cases at post-mortem examination was there seen any injury to the os uteri beyond the ordinary fissuring that would take place in first labours." Accept this, as I do, without reserve; still, that an undue degree of contusion was caused, that suffering and shock from the violent derangement of the process of labour were increased, and that these conditions may have influenced the mortality, we may be permitted to suspect.

A most serious objection to the high operation lies in the difficulty of diagnosing whether the case before us is one that fairly admits of the operation being carried out. There are, for example, minor degrees of obstruction, from slight pelvic contraction, from rigidity of the cervix uteri, or from slight excess in size or want of plasticity of the child's head, which the long forceps may succeed in overcoming. We are, in fact, on the very border-line of the dominion of the forceps; and we cannot always tell, until we try, whether the forceps is equal to the trial or not. In the case of primiparæ this point is especially difficult to determine. It may to some extent be determined—first, by exploring the brim of the pelvis fairly by the hand under anæsthesia; and, secondly, by a tentative use of the forceps. We can but give it up for turning or craniotomy if it fail.

We may do much to overcome rigidity of the cervix uteri by copious warm water irrigation, and the hydrostatic bags.

There is an argument which the advocates for the free use of the forceps urge, and justly urge, which claims the most earnest attention. Neglect of the forceps entails abuse of craniotomy. You wait so long that at last the child has to be sacrificed in order to save the mother. A careful survey of the annals of obstetric practice justifies this conclusion. I have drawn up a table to illustrate this point. Thus Collins's craniotomy cases were 1 in 211, whilst his forceps cases were 1 in 607; Ramsbotham's craniotomy cases were 1 in 802, his forceps cases 1 in 670; Johnston's craniotomies were 1 in 281, his forceps cases 1 in 10½. These refer to the ordinary run of public practice. But if we take the records of the most ardent opponent of the forceps, the late Robert Lee, we shall have the most striking example of what a doctrine carried out to the most rigorous extreme will lead to in practice. During an active consultation practice ending in 1863, Dr. Lee performed craniotomy in 186 cases, and used the forceps only in 53. He never used the long forceps. No one can peruse the histories of his cases without the painful conviction that in many instances the long forceps might, with advantage to the mother as well as to the child, have been substituted for craniotomy. It is true that this terrible experience is the record of consultation practice. He was commonly called in to cases which had drifted within the confines of danger. We must further remember that he practised during a reign of terror, which he himself had done more than any man to create, when to use the long forceps was held to be almost a criminal act. But making the most liberal allowance for all this, admitting that he would meet with a larger proportion of desperate cases than can possibly occur again, the preponderance of the perforator over the forceps will still strike us as appalling.

I may here state that my experience of the comparative resort to the forceps and craniotomy, and of the mortality of mothers and children in the practice of the Royal Maternity Charity, is very similar to that of Ramsbotham, whom I succeeded; and I think I may appeal to my successor, Dr. Roper, to support the proposition that the statistics of this great institution still bear the same evidence.

I regret that leisure utterly fails me to draw out an analysis of my forceps and craniotomy operations during an active consultation-practice of twenty-five years. But I may safely venture on the statement that my forceps and turning cases greatly outweigh the craniotomies. Contrasted with craniotomy, the forceps is frequently an elective alternative operation. And so, again, turning is not seldom an elective alternative against craniotomy. To have demonstrated this I take to be one of the many claims to the gratitude of mankind, and to a place in Westminster Abbey, which Sir James Simpson has established.

Taking a scientific point of view, craniotomy should never be the alternative for the forceps. The dominion of this sacrificial operation is totally distinct from that of the conservative operation of the forceps. To apply a recent famous political expression, we may take it that there is a "scientific frontier" against craniotomy. We have not yet secured this frontier. We may never acquire it absolutely. But it nevertheless exists potentially; and it is our duty to strive after it by constantly advancing the outposts of the forceps and of turning.

I beg leave to conclude this rapid and inadequate review of a difficult question by stating the following propositions as presenting the points that chiefly challenge discussion:—

1. In lingering labour, when the head is in the pelvic cavity, the forceps is better than its alternatives.

2. In lingering labour, when the head is engaged in the

pelvic brim, and when it is known that the pelvis is well formed, the forceps is better than its alternatives.

3. In lingering labour, when the head is resting on the pelvic brim, the liquor amnii discharged, and it is known, either by exploring with the hand or by other means, that there is no disproportion, or only a slight degree of disproportion, even although the cervix uteri is not fully dilated, the forceps will generally be better than its alternatives.

Table showing relative frequency of Forceps and Craniotomy Operations, and the Mortality of Mothers and Children.

	Total labours.	Forceps cases.	Craniotomy.	MOTHERS. Total deaths.	CHILDREN. Total still-born, excl. premature.	Proportion of forceps to total.	Proportion of craniotomy to total.
Collins (1829–1833), Rotunda Hospital	16,414	27 (4 deaths)	79 (15 deaths)	164 (Prop. 1·0)	823 (Prop. 5·0)	1 in 607	1 in 211
Ramsbotham (1828–1850), Royal Maternity Charity	43,996	73 (3 deaths)	60 (6 deaths)	220 (Prop. ·41)	1822 (Prop. 3·7)	1 in 670	1 in 802
Dunn (1830–1850), London private practice	4,049	20 (0 (!) deaths)	10 (2 deaths)	27 (Prop. ·67)	170 (Prop. 4·1)	1 in 202½	1 in 404½
Johnston (1868–1875), Rotunda Hospital	7,862	752 (58 deaths)	28 (7 deaths)	179 (Prop. 2·3)	470 (!) (Estimated 6·1)	1 in 10½	1 in 281

Robert Lee's Consultation Practice (London).

	Total forceps.		Craniotomy.	
		Died.		Died.
First Period, "Clinical Midwifery," ending 1848	48	11	95	14
Second Period, "Consultations in Midwifery," ending 1863	5	1	91	5
Totals	53	12	186	19

Dr. Lee's book gives 80 forceps; 32 are deducted, either because R. Lee disapproved of operation or finished by craniotomy. Thus R. Lee's forceps cases stand to his craniotomy cases as 1 to 3½.

4. In proportion as the head is arrested high in the pelvis, in the brim, or above the brim, the necessity, the utility, and safety of the forceps become less frequent.

5. As a corollary from the preceding proposition, increasing caution in determining on the use of the forceps, and greater skill in carrying out the operation, are called for.

In most things there is a middle way. "Ni jamais, ni toujours," is a proverb full of wisdom. I cannot better illustrate the wisdom of deducing the greatest good from over-caution on the one hand and from too bold enterprise on the other, than by citing the precept and practice of Boër. This famous surgeon, having witnessed in Paris the extreme activity of French midwifery, and in London the too procrastinating practice of England, recognised the middle course as the best, constructed his forceps of medium length, saying, "Everything is not to be taken away from Nature, neither is everything to be left to her."

LITHOLAPAXY.

BY HENRY J. BIGELOW,

PROFESSOR OF SURGERY IN HARVARD UNIVERSITY, SURGEON OF THE MASSACHUSETTS GENERAL HOSPITAL.

THE LANCET of February 1st contains a paper by Sir Henry Thompson, entitled "A Lecture on Lithotrity at one or more Sittings." In another communication, Feb. 15th, he promises to give his views more completely "in the fifth edition" of his Clinical Lectures "now going through the press." The publication of this volume will be looked for with the more interest because the Lecture on Lithotrity fails to give a clear exposition of the author's existing views. It mixes new and old ideas, its tendency being to obliterate rather than define the line between what has been done by lithotrity hitherto and what can be accomplished now; leaving the reader uncertain how far Sir Henry discriminates between the new and the old method.

The new operation enlarges the range of lithotrity, and encroaches upon that which has hitherto been regarded as belonging exclusively to lithotomy. This is all I claim for it. Cases can be relieved by litholapaxy which would not have been treated by the old lithotrity; as, for instance, one case, where eighteen hundred and two grains of soft stone were removed at three sittings, one of nearly four hours' duration, the patient travelling home four days afterwards. Such a result, till now wholly unprecedented, would have been before regarded as bordering on the impossible. Experience alone can decide the limits of the new operation.[1] I am surprised that Sir Henry should attribute to me a disposition "to make the rule absolute to remove at one sitting an entire stone, no matter how large it may be, or what may be the condition of the patient." He adds: "Invariable conformity to such a rule, I do not hesitate at the outset to say, will lead to results which, though often successful, will not seldom be disastrous." This is obvious.

In attempting to identify the old operation and the new, Sir Henry relies mainly upon his having used Clover's form of Crampton's instrument, with which Sir Philip, before 1846, drew "upwards of two drachms of pulverised calculus at once from the bladder."[2] He dwells much on what he calls "that useful instrument, the aspirator of Clover." It is figured in his former works with half a dozen differently curved catheters attached to it. He now figures it (see THE LANCET) with "a slight modification," as he says, based upon what he calls a "hint," from my "aspirator." It is quite extraordinary that Sir Henry should claim such efficiency for Clover's apparatus. Except in cases where the

[1] So early as 1846 Sir Philip Crampton said: "It appears, then, that cystotomy and lithotrity are not to be considered as rivals, and that the question as to which of the operations should be the rule and which the exception should never be brought into discussion; each operation has its special province, the boundaries of which (if, indeed, they admit of being fixed at all) can be determined only by a comparison of a vast collection of facts, carefully noted, and, above all, faithfully reported and properly authenticated." (Dublin Quar. Jour. of Med. Sci., vol. i., 1846, page 25.)

[2] Op. cit., page 22.

prostate is so enlarged that the bladder retains even sand, his is the only voice, so far as I know, that has spoken in its favour.[3]

Clover's instrument is a very good bladder-washer. But as an evacuator of anything but sand and minute fragments that can also pass the urethra without it, it is unequivocally worthless. It cannot be otherwise. Its size, with its collar, is only No. 21 French calibre, through which nothing of importance can escape from the bladder. In fact, to adapt it to the new system, Sir Henry has been compelled to enlarge its calibre from 21 to 26. Again, the bulb of Clover is so weak that in a vertical position it cannot expand to its own full dimensions, much less can it aspirate effectively. For this slender bag Sir Henry has now substituted a bulb, "stiff," as he describes it, like mine. The short curve which I had selected from the arsenal of old catheters and sounds for the end of my curved tube he now discovers to be the "more generally useful form."[4] He has also adopted my terminal trap below the bulb. With these modifications he gives what he entitles "an admirable illustration of the existing method." This flattering adoption of the essential features of my plan, which Sir Henry calls "taking a hint," is rather like taking the apparatus. If not, let Sir Henry endeavour to evacuate a stone of moderate size with "Clover's original instrument" as now "slightly modified" by him, omitting only the "stiff" bulb and enlarged tube.[5]

Although my pump acts perfectly, the operator will doubtless modify it a little to suit himself, adopting the necessary conditions of an adequate exhaust of some sort, an adequate evacuating tube, and an efficient trap. I find also that an elastic tube interposed between the bulb and the bladder, to avoid the jar of pumping, is desirable.

But what I insist upon, as characterising the new lithotrity, is :—1. A practical recognition of the tolerance of the bladder, which is far less sensitive to instruments than to fragments,—with the obvious corollary, that, when the operator crushes a stone, it is better to remove the fragments by protracting the operation, than to leave them, as has been hitherto the usual practice of distinguished operators, including Sir Henry Thompson; and (2) the use of a tube large enough to afford an easy passage from the bladder, not (as with Clover's instrument) for sand, which does not need it, but for detritus, which, owing to its size or quantity, would not pass the urethra without it; this being an application of Otis's discovery of the previously unrecognised capacity of the urethra, of which the meatus is the narrowest part.

This is not in accordance with the former teaching of lithotritists. At the late lithotrity meeting of the Medico-Chirurgical Society there was no question about the traditional few minutes,[6] as the limit of time beyond which it is not usually safe for the lithotritist to try the patience of an average bladder. The error is a natural one. Surgeons have hitherto attributed to instruments of polished metal the damage really due to the roughness of fragments. With Sir Henry Thompson, they believed that "the mere sojourn of the instrument in the bladder is a source of irritation precisely corresponding to the time, within certain limits, it continues there. Anything, therefore, that will diminish the time of the operation, and the amount of movement and concussion, will necessarily give a greater prospect of success."[7] This mistake prevailed for half a century, because the real cause of the injury could not be ascertained. There was absolutely no way to extract fragments large or abundant enough to make trouble, and to show what would be the behaviour of the bladder when thus relieved. The new instrument first accomplished the evacuation of bulky débris, and by removing this source of inflammation disclosed the comparative harmlessness of the lithotrite and the pump. The discovery of the tolerance of the bladder could have been made in no other way. What Sir Henry Thompson would call a little modification of method involved an important difference in result, making it easy to accomplish what was before impracticable: a trite occurrence in the history of inventions.

There can be no doubt about Sir Henry Thompson's teaching. So lately as 1876 he writes, "You heard me say in the theatre the other day ..... that you should not encourage the early passing of the fragments. They rest at the bottom of the bladder. I usually keep the patient in bed, and pretty much on his back, for thirty-six hours or so, afterwards; he should, at all events, for that period of time, pass urine in that position, so that the sharp angular fragments are left at the bottom of the bladder, and are not forced into the urethra."[8] With unparalleled opportunities for observation, Sir Henry seems to have been well satisfied with the old lithotrity and its instruments. In the lecture to which he refers for an exposition of his latest views, he says, "I do not mean to say that any striking novelties in the instruments, or in the system of operating, have recently been achieved. The mechanical procedure has probably long been too nearly perfect to permit us to expect results of that kind."[9] When this conservative view was again expressed at the lithotrity meeting of the Medico-Chirurgical Society in 1878,[10] it elicited nothing from Sir Henry concerning the tolerance of the bladder to instruments, the advantage of removing all fragments, or any other essential feature of the new operation.

I may here allude to a small matter. Sir Henry insists upon the importance of making the movements of the bulb keep time with the respiration of the patient. I think this will be found both unnecessary and impracticable. The first part of any long aspiration, whether it is or is not synchronous with the breathing, suffices to clog the orifice of the tube with débris; the rest of the aspiration is then useless. Nor does a "stiff" bulb need to be aided by the diaphragm of the patient. Nor need danger be apprehended from any expulsive effort to which the bladder is accustomed, like respiration, cough, or even vomiting. In fact, this last is not unusual with an anæsthetic: I let the water off while it continues.

Although a long stroke is occasionally useful, I commonly pump at the rate of about twice in three seconds, moving only about two ounces of water back and forth. The main difficulty is to find for the extremity of the tube the best place for gathering fragments, and to prevent its obstruction, whether by fragments or by the bladder. This somewhat nice adjustment is experimental, and, I think, is easiest

---

[3] For the information of those who have not seen my paper, I here cite the latest authorities on this point:—"We may here say, without fear of being accused of exaggeration, that evacuating injections practised after sittings of lithotrity have no apology for their use. The whole surgical arsenal invented for their performance is absolutely useless. .... It should be well understood that the best of evacuating catheters is worthless." (Article "Lithotritie," by Demarquay et Cousin, in the Nouveau Dict. de Méd. et de Chirur. Prat., pages 693, 694; Paris, 1875.)—"Having used it (Clover's apparatus) very frequently, I would add that it is necessary to use all such apparatus with extreme gentleness, and I prefer to do without it, if possible." (Sir H. Thompson, "Practical Lithotrity and Lithotomy," page 215. 1871.)—"All these evacuating catheters are little employed. They require frequent and long manœuvres, which are not exempt from dangers; besides, they give passage, as a rule, only to dust, or to small fragments of stone, which would have escaped of themselves without inconvenience to the urethra." (Article "Lithotritie," by M. Voillemier, Dict. Ency. des Sci. Méd., page 733. 1869.)—"In short, the 'evacuating apparatus' and the evacuating method hitherto employed do not evacuate. This fact is beyond question." (Litholapaxy, &c., by Henry J. Bigelow, pages 6, 7. London: Churchill. 1878.) Sir Henry Thompson also testifies to the inefficiency of Clover's instrument in admitting that when, in the old lithotrity, he wanted to evacuate the fragments as rapidly as possible, he employed several sittings. "It is desirable," he says (Clinical Lecture, THE LANCET, Jan. 8th, 1876, page 38), "to treat any cystitis that may occur during the course of the sittings, especially if it is severe, by freely crushing the stone without delay. .... Under these circumstances," he continues, "I place the patient under the influence of ether, crush freely all the large and sharp pieces, and wash out the débris with Mr. Clover's aspirator." But as Clover's instrument evacuates inefficiently, Sir Henry is compelled to add, "If some large pieces still remain after the fresh crushing, the former symptoms of cystitis reappear in two or three days, and may be again relieved by another sitting, this time altogether or nearly so, as a succeeding crushing will, no doubt, dispose of the principal part of the stone, and leave, at all events, no considerable fragments."

[4] Hitherto Sir Henry has designated, as the most useful form, a squarely cut extremity. (The "Diseases of the Prostate," &c., by Sir Henry Thompson. 4th Ed., p. 337. Philadelphia: Henry C. Lea. 1873.) "In most cases," he says, "the best kind is that which is cut transversely at the distal end." Such an extremity is really the worst, because it is at once closed by the bladder.

[5] Sir Henry does not seem to appreciate the full use of a large tube. This is shown by his figure of the "modified" instrument (see THE LANCET, February 1st), where, although the calibre of the evacuating tube is 26, its hole for the admission of fragments is but little more than half its diameter. Where lies the utility of so large a tube, if only small fragments can get into it? This hole is a relic of the Clover instrument. As regards the size of the tube, the operator can fairly take advantage of the full size of the urethra. The larger the tube the larger will be the fragments released. I have employed, without any objectionable result, a 30 French calibre, often 31, and I prefer a straight tube. Although these largest sizes are not essential to the operation, they admit many fragments that would otherwise have to be crushed.

[6] "A sojourn, say of two minutes, in the bladder, which I will allow you, although you know I do not occupy so much time myself." Clinical Lectures on Diseases of the Urinary Organs, &c., by Sir Henry Thompson. 4th Ed.; page 188. London: Churchills. 1876.

[7] Op. cit., page 172.

[8] Op. cit., page 189. See also page 190.

[9] Clinical Lecture, LANCET, Jan. 8th, 1876.

[10] THE LANCET, March 16th, 1878, p. 385.

when the left hand, hold the evacuating tube, rests on the pubes. The manipulation is provided for in my instrument by the intervention, between the evacuating tube and the bulb, of an elastic tube eight inches long. If the operator prefers, it may be shorter; my first one was only two inches long. The facility and delicacy of the manipulation, it is needless to say, are greatly impaired if the bulb be rigidly attached to the evacuating tube, as in Clover's apparatus and Sir Henry Thompson's figure,—especially if, as there, it is placed above the tube.

In my present method of evacuating the débris, the bulb full of water is coupled with the evacuating tube introduced into the empty bladder, and when the pump is in action, water is added to distend the bladder, if its walls prove to be slack enough to fall into the eye of the tube and obstruct it. The earlier cases naturally occupied more time than is now necessary, but I still find, as at first, that the most time is spent in searching for and removing the last fragment, and in incidental delays, not in evacuating the mass of débris, which is drawn out with surprising rapidity. With ether, rapid surgery has become less essential, and lithotrity is now, perhaps, less than any other surgical operation, to be performed against time. I doubt whether the hurried operation mentioned by Sir Henry Thompson would bear frequent repetition even by himself. There would seem to be need rather of deliberation than of haste. A remaining fragment may be easily overlooked, and in less skilled hands haste might be mischievous. Removing a stone of 120 grains, with two lithotrites and an assistant to clear them, is mainly the operation of Fergusson, who relied for evacuation on repeated withdrawals of the loaded instrument.

Litholapaxy is now no novelty in America, and its success here will, I think, recommend it elsewhere. Let me add a few recent examples.

An operation which I performed January 26th, in the case of a medical gentleman, aged sixty-seven years, lasted fifty minutes, and consisted of two crushings, occupying fifteen minutes; three evacuations of fragments, nine minutes; changes and other delay, twenty-six minutes. Two hundred and sixty grains of phosphatic stone were thus removed. The patient had no trouble from the operation, and on the thirteenth day went home to the country, well. There were no fragments left in the bladder.

In another case (February 10th), that of a man aged fifty years, one diameter of the stone measured 1⅛ inch. The operation lasted one hour and twenty-one minutes. The crushings occupied twenty minutes, the evacuation of fragments thirty, while the changes &c. were recorded at thirty-one minutes. Three hundred and two grains of hard oxalic calculus were crushed and drawn out,—with some delay in the operation, due to fragments lodged behind a high prostate. I was unable to break the stone with Charrière's, or rather Collin's, instrument. The patient had no unfavourable symptoms, hardly a trace of blood, and no fragments were left.

This case, which involves, so far as I know, the largest hard stone yet evacuated at one sitting, is an example of what can be done by the new process. In evacuating such stones, it need only be said that, the smaller the tube the more minutely must the fragments be broken, and the greater will be the liability to obstruction. Small stones, common in these later days of lithotrity, especially soft ones, are not unfrequently crushed at one sitting, by any lithotrite, without ether, and, if reduced to sand, may really need no tube to evacuate them.

The following case is as good a test of the new operation as I could wish. The patient, aged thirty-three, entered the hospital October 31st, about four months and a half ago. His condition was so bad that it was thought unadvisable to attempt any operation, even lithotomy. The urine was ammoniacal and fetid, always containing a large quantity of blood, also pus and mucus to the amount sometimes of nearly one half by measurement. Micturition was very frequent, occurring at intervals of from ten minutes to half an hour, day and night, during much of this time. The straining was excessive, ineffectual, and productive of great suffering. Three unsuccessful attempts having been made on previous days, a sound was first introduced into the bladder, under ether, November 10th. The next day the temperature rose to 103°, and remained thereabouts till the fourth day, when another complication presented itself. The left knee became suddenly inflamed and swollen. It has remained so ever since. During the next two months the temperature ranged from 100° to 102° daily—afterwards slowly receding, though the other symptoms did not abate. I saw the case, for the first time, March 7th. With so diseased and irritable a bladder, it was evident that litholapaxy could be considered only as an experiment. It was a last resort, being perhaps better than lithotomy. Should it succeed, it would testify strongly in favour of the new method; should it fail, it could hardly be counted against it. On the 9th of March I operated. In the neighbourhood of the triangular ligament an obstruction prevented the passage of sounds larger than a No. 15 French calibre. After snipping the meatus, this obstruction was divulsed by Voillemier's instrument, and it then admitted a full-sized lithotrite, and a straight tube 29 French, for which, later in the operation, 30 was substituted. Two hundred and forty grains of stone were now slowly and carefully removed in sixty-eight minutes. An abundance of flocculent and fibrinous material concealed the fragments when lying in a basin, and testified to the inflammation. At 4 P.M., four hours after the operation, the temperature had fallen from 99° to 96°. In eight hours more, at midnight, it had risen to 103°, with a pulse of 130, where it remained through the day, the tongue being red, smooth, and dry. A general pain in the region of the bladder and urethra required opiates. Yet on the third day the tongue became moist, with a light coat, the temperature had fallen to 99°, and the pulse to 84. This improvement still continues. The patient has had no such comfort for many months. During the first week after the operation, he passed his water six times in twenty-four hours almost without pain, and there has been no tenderness over the bladder. The urine contains very little sediment, and, apart from the knee, which remains as it was, the patient is rapidly convalescing.

I have only to add a few words regarding the new lithotrite. Sir Henry does not seem so heartily satisfied with mine as I could wish, nor, in fact, with anything connected with the new operation, unless possibly its results. This lithotrite, he says, is "surely some resuscitated relic of the early history of lithotrity, ... reminding one very forcibly of the terrible engines used by Heurteloup."[11] The present tendency of London lithotrites is to small size, because it has been believed that the smaller the instrument the less the danger. With long operations, large stones, and the general use of ether, they will again be larger. With size we gain power. There is no more reason for employing small lithotrites to empty, under ether, an average bladder, than small catheters to draw the water. In fact, the surgeon who becomes accustomed to the efficient action of a large lithotrite does not willingly relinquish it; a little more care is necessary in introducing it, but, if non-impacting, it need not be withdrawn, loaded with sharp fragments, through the neck of the bladder, at short intervals, to be cleaned by an assistant.

My new lithotrite proves to be very efficient, and I am recently indebted to London makers (Weiss and Son) for an instrument that works perfectly. It is of a good size for general use; a smaller one, if preferred, may be used in special cases. The instrument is non-impacting, and keeps clean in the bladder for an indefinite time. Its rounded tip protects the bladder in a protracted operation,—as it also does the prostate during introduction. For the old wheel, which hurts the hand in long crushing, the ball is a welcome substitute. And unless the human hand undergoes some modification of what are now its easiest movements, the system of a *right-hand lock*, here first employed, must, as I believe, whatever be the size of the lithotrite, supersede in time any previous method of locking.

Boston, Massachusetts, U.S.A.

[11] Of Heurteloup's operation Crampton says, "Nothing could exceed the dexterity and skill, unless it be the gentleness, with which it was performed." Op. cit., page 17.

ADULTERATION IN DUBLIN.—During the past year 1690 articles were examined by the city analyst, of which number 44 were found to be adulterated. The milk samples had an addition of from 8 to 50 per cent. of water, and in two instances of rice flour. The coffee was adulterated with burnt biscuit, burnt sugar, and chicory; the mustard with a large quantity of wheat-flour, cayenne pepper, and turmeric; the arrowroot with potato starch; the brandy with corn spirit; and the butter with foreign fats.

THE OSSEOUS LESIONS OF HEREDITARY SYPHILIS.[1]

BY M. J. PARROT,

PROFESSOR OF THE FACULTY OF MEDICINE, PARIS; PHYSICIAN TO THE HOSPICE DES ENFANS-ASSISTÉS, ETC.

IN the account of the osseous lesions of hereditary syphilis which I have the honour of bringing before the Pathological Society, I shall not relate the history of the question. Upon that the honourable members of this Society know as much and more than myself. In responding to their kind invitation, I wish simply to bring forward the result of my personal researches on this point of pathological anatomy, whilst exhibiting in support of them several typical specimens from my own collection.

Hereditary syphilis attacks, with a manifest predilection, on the one hand the skin—that is to say, the most superficial, most sensitive, and most vascular of organs; on the other hand the skeleton, buried deeply in the flesh, but slightly sensitive, and little vascular. The viscera and the muscles, with their appendages, only come, as regards frequency of lesions, after the external tegument and the bones, and, I may add, very far behind them. I shall only deal here with the changes of the osseous system.

I long thought that these changes were constant, and that consequently they were the most frequently met with of any in the organism. At the present time I could not be so positive, and since (by means of the scars left behind by syphilides) I have learnt to recognise the frequency of changes in the skin, I am quite disposed to admit that the latter and the skeleton are attacked very nearly in the same proportions—i.e., in the immense majority of cases. Besides, although very exceptionally the autopsy reveals no actual lesion of the bones, it may not be concluded that they have not been attacked previously, or that they would not have been subsequently; so that I still think that if my former dictum is not the precise truth, it comes very near to it.

The syphilitic changes of the skeleton are extremely various, and lead to such considerable alterations in form, colour, consistence, and structure of the organs attacked that their classification seems at first sight to be very difficult. However, the comparison of a large number of facts reveals points of contact and actual bonds of union between lesions apparently widely separated, and allows of their being classified in a methodical and simple manner. The best way not to go astray in this study is to never lose sight of the age of the patients, which is also the age of the disease. It is rare for the whole skeleton to be attacked; and certain parts are much more often so than others. These are the long bones of the limbs, and the maxillary bones; then those of the skull-cap, the ribs, the scapulæ, and the iliac bones. The vertebræ and the different bones of the foot and hand are less frequently attacked. Here I shall only deal with the bones of the limbs, notably of the humerus and tibia, as well as with the skull. Their lesions have a characteristic seat and appearance. They are of two chief kinds; one consisting in the atrophy of pre-existing tissues, the others in the development of new products.

Atrophy.—This assumes two forms. The first—much the more frequent—occurs equally on the skull and on the limbs. I have termed it *gelatiniform*, because in fact the parts attacked take on the appearance and consistence of a very aqueous fruit jelly. It is marked, to begin with, by a modification of the normal tint, which varies from garnet to rose, and from a deep citron to a bright maize colour—these different shades being moreover met with on the same bone. The marrow is glistening, transparent, and in it there occurs a gradual diminution of medullary cells, which may finally even wholly disappear, and there is found only a vasculo-fibrillar meshwork, and much water. When the hard parts are invaded, they are rapidly decalcified, the spongy lamellæ literally melt away, and are replaced by large spaces containing a tissue which has the appearance and histological structure just indicated in the altered medulla. I call the second form of atrophy *chondro-calcareous*. One knows that between the osseous diaphysis and the cartilage there is normally a layer of cartilage encrusted by lime salts. I term this the chondro-calcareous, and M. Ranvier the *ossiform*, layer. It is very thin, for it does not exceed one millimetre in thickness. In syphilitic subjects it attains an unusual thickness, and loses all regularity in its boundaries. The chondro-calcareous tissue, when existing in these proportions, denotes an arrest of ossification. The portion of cartilage bordering on the diaphysis, instead of being transformed into bone, remains in the state of cartilage, but of cartilage hardened and eburnated by the presence of calcareous salts. The vessels disappear; and instead of the osteoplasts, which characterise osseous tissue, only cartilaginous capsules are found, more or less altered, and filled with nuclear corpuscles. The diseased tissue extends from the cartilage towards the spongy part of the bone, from which it differs by a greater density and friability, and by a white colour, like chalk, which it resembles in many respects. Sometimes it occurs in isolated nodules in the centre of the spongy lamellæ, or indeed in the centre of a gelatiniform mass; for both sorts of atrophy may exist at the same time and in the same region.

Both these lead to a diminution in the solidity of the bones which they attack. The chondro-calcareous tissue is indeed much more friable than normal bone; and it is needless for me to insist on the softness of the gelatiniform substance. Each of these changes may then determine, and in fact does determine, fracture of bones; and when they occur combined, such a result is very nearly certain to take place. This fracture of bones, which is different in many respects from that observed in the rachitic period of syphilis, differs from the latter especially in its seat; for whilst in this latter case it takes place very nearly at the middle of the bone, in the other it occurs not far from the epiphysis (sometimes from one to two millimetres, sometimes one centimetre, from the cartilage), this being in relation to the place where the atrophic lesions producing it are developed. When the separation of the fragments is complete, or even very advanced, their friction produces irritation, and occasionally intra- and peri-osseous abscesses, and suppurative arthritis. These fractures have consequences clinically full of interest, and may be diagnosed during life. They produce, indeed, quite a special form of weakness, which I have termed "syphilitic pseudo-paralysis," because it simulates paralysis of nervous origin, and because it has been often confounded with such. The little patients possess an inertia, the degree of which varies with that of the lesion. In some cases it is difficult to ascertain; in others it is absolute. There is little pain. As a rule, the thoracic limbs hang down by the side of the trunk in a state of pronation. The pelvic limbs are elongated, contrary to what takes place in healthy subjects; and when the child is raised they hang and oscillate on the slightest shake. The muscles respond completely to electrical excitations. When there are abscesses, or notable displacement of the bony fragments, swellings are met with in the vicinity of the joints. It is possible in some cases, by manipulating the fractured limbs, to produce genuine crepitation. This phenomenon, together with the preservation of muscular contractility, suffices to establish the diagnosis.

The second form of syphilitic change of the skeleton, that which I call *osteophytic*, is much the more frequent. It consists essentially in the development on the surface of bones of new products in various proportions, modifying their size and causing them to undergo diverse but truly typical deformities. Two varieties must be considered, according as the tissue presents a bony hardness, or as it is fibroid and spongioid. I term the first, which may occur at all ages, the *osteoid* variety, and the second—which is never developed except in subjects more than five or six months old—the *rachitic*. To these principal alterations there are connected others which I shall incidentally describe, but which we ought to guard ourselves from making the criteria of subdivisions, for by so doing we should run the risk of great confusion. They result from an accessory morbid process which takes place together with the chief process, by it and because of it.

The osteophytes, whatever their consistence and structure, follow with tolerable regularity certain topographical laws. Amongst the bones of the limbs, those in which the change occurs most frequently and regularly are, firstly, the humerus and the tibia, which deserve a place completely apart; then the femur and the ulna. When the whole of the skeleton is involved these bones are much more affected than the rest, and when the diathesis is not of great degree it may

[1] Communicated to the Pathological Society of London on May 6th, 1879.

happen these bones are alone diseased. On the humerus without exception (as is readily made out by means of an antero-posterior section through the length of the bone) it is the lower part of the diaphysis which is attacked, and in a much more marked manner the nearer the cartilage is approached, the change thinning out above. I have seen it in a certain number of cases completely ensheath the diaphysis. It is in the posterior aspect that the thickness of the osteophyte is most considerable. It may vary from one and a half to two, or even three or four millimetres. On the tibia the osteophyte is very frequently, if not exclusively, developed on the inner aspect, and its greatest thickness is at the middle part of this surface. To see it clearly the bone must be cut longitudinally as well as transversely. The seat of predilection of the osteophyte is, for the femur, the antero-external region; for the scapulæ, the supra- and infra-spinous fossæ; and, for the iliac bones, the external iliac fossa. The other long bones present no notable peculiarity, for the seat of the osteophytes in them is much less constant; however, in a general way it may be said that the pathological layers are especially thickened and ensheathing in that diaphysial extremity which grows the less rapidly—such, for example, as the upper end of the ulna and radius. Occasionally the osteophyte is so intimately united to the diaphysial surface that there is some difficulty in distinguishing it. But in the greater number of cases the line of demarcation is clear enough; it is enlarged even into a true medullary space. It is especially characterised by its structure, as apparent to the naked eye. It is, in fact, formed by trabeculæ which have a perpendicular or a very slightly oblique direction to the axis of the diaphysis. It may only be formed by a single layer, but in many cases several layers can be distinguished, differing in colour and texture, and often separated from one another by medullary furrows or spaces, in which small transverse trabeculæ can usually be made out, being undoubted relics of an osteophytic deposit for which marrow has been substituted. The periosteum, nearly always thickened, and that in a very marked manner, is applied over the osteophyte, and adheres to it much the more firmly the more this departs in structure from that of the bone. The constant seat of the osteophytes allows of their recognition during life, and they thus form a very valuable element in diagnosis, for there is no other diathetic condition which can give rise to them. The method of examination is very simple. In examining the humerus, it suffices to seize its lower end between two fingers, so that one is applied on the anterior and the other on the posterior surface. Then, when the children are not too fat, which is most usually the case, it is found that this extremity exceeds its normal limits in thickness. For the tibia the ball of the finger must be passed along its inner surface, which, instead of being flat, presents a kind of arch.

The following is briefly the structure of the osteophytes, and here it is that the distinction comes in which I have above made between the *osteoid* and the *rachitic* varieties. The former may exist at all ages, and before the age of six months they alone are met with. They are formed by trabeculæ more or less regular and numerous, interlaced and directed perpendicularly to the diaphysis; infiltrated, like it, with a large amount of calcareous salts, and surrounded by medulla. They only differ from normal bone in a more yellow or more rose colour, and especially in a greater friability and the readiness with which they can be punctured or cut. The microscope shows that they have not a systematic structure, like true bone; there are not to be seen in them, as in the latter, osteoplasts regularly disposed in the fundamental substance around Haversian canals; there are triangular or polygonal corpuscles, recalling the stellate corpuscles of connective tissue, anastomosing by means of processes springing from them with the periosteum, whence they emanate, and with the analogous corpuscles existing in the medullary spaces. In the spongy or rachitic form, which occurs very exceptionally before the age of six months, the osteophyte is formed by tissue which M. Jules Guérin, and after him M. Broca, have styled *spongioid*. It is nearly white, and of a pearly or slightly yellowish appearance. Its structure is essentially fibroid. It is slightly vascular, and contains but little marrow. To these two principal forms must be added a large number of intermediary varieties, from that in which the osteophyte, instead of being simple, is composed of many layers of variable structure and consistence, but always so disposed that the harder layers—those containing most calcareous salts—are the most central, that is to say, the nearest to the diaphysis; whilst the most spongioid and most deprived of calcareous matter occur at the periphery immediately beneath the periosteum. And it can be seen, from this disposition, that the second form is derived from the first, since between them can be marked a series of intermediary conditions which pass insensibly from the one to the other. During this time the diaphysis is but slightly modified in very young subjects with osteoid osteophytes; but in others it becomes decalcified, splits up into parallel lamellæ by the development of furrows filled with marrow. Lastly, it may undergo the spongioid transformation, and it then becomes extremely light, porous, and friable; and when it has lost its soft parts by maceration, it has, so to speak, neither weight nor form. With the osteoid osteophytes the cartilage is but little changed; the chondro-calcareous layer only is thicker and more apparent; but in proportion as the change tends towards the rachitic form, the chondroid layer increases, and eventually even takes on considerable proportions, becomes filled with outgrowths of the diaphysial spongioid tissue, and is vascularised in a very marked manner. There thus results a swelling, sometimes of enormous size, of the articular ends of the bones; and that forms one of the best signs of the rachitic variety of hereditary syphilis.

These changes in the skeleton lead to grave results. Decalcification, lamellation, spongioid transformation, diminish and even destroy its solidity, and render it unfit to fulfil its principal function, which depends on its resistance and rigidity. Hence occur curvatures, partial or complete fractures, deformities of all kinds of the limbs, the vertebral column, or thorax. Then ensue disturbances of functions, sometimes of considerable degree. Walking is retarded or impossible; the movements of the lower limbs are difficult; abnormal attitudes are produced; respiration is difficult, because of the flexibility and fracture of the ribs, and of the slight resistance offered by the thoracic cage to the action of weight. In one word, there is a state of powerlessness, which in cases where the disease is extreme attains dangerous proportions.

This is what may be said of the course of the lesions already described, at least in part, and upon their termination. So long as the diathesis is active the osteophytes increase at the periphery, whilst often they are destroyed at the level of the preformed parts, replaced by medulla. In the period of repair, when all tends to return to the normal, at the same time as the primitive bone with which they are perfectly combined, and the nutrition changes of which they share in, they tend to take on all the structure, appearance, and attributes of healthy bone, and that by means of a simple enough series of changes. In the greater number of cases, by reason of those changes, which the evolution of the skeleton (so rapid at this age) actively favours, all trace of the disease disappears. But it is not always so, and there are subjects upon whom indelible marks of the disease remain. In this category are torsions, curvatures, fractures with all their consequences, and even osteophytes, notably when these are seated on the cranial bones.

The cranium undergoes the changes which have just been described, but in proportions and with peculiarities special to it. Gelatiniform atrophy is rare in it, yet it is to be seen in very young infants, and I am inclined to believe that it may commence in intra-uterine life. It begins, without exception, peripherally—that is to say, under the periosteum, and only reaches to the dura mater in very rare cases, and when the disease is intense and rapid. I have only once seen that take place, and at the seat of perforation there was developed a partial meningitis. Commonly, the lesion, whether circumscribed or diffuse, is not very deep. To appreciate it well, the specimen must be dried immediately or after maceration. Then are seen the thinned-out surfaces resembling wormeaten wood or cloth. More rarely the loss of substance appears just as if it had been made with an *emporte-pièce*. All the bones of the skull-cap may be attacked. The diagnosis of this lesion cannot be made during life.

The cranial osteophytes have a great practical interest. Never observed at the same time with gelatiniform atrophy, they are only developed in older children. They occur very nearly exclusively on the periphery, and at the commencement always in the same points—viz., the peribregmatic regions (i. e., around the anterior fontanelle) of the frontal and parietal bones. More rarely they start from the temporals. They are very rare on the orbital arches and the occipital bone. They are only observed when the disease is of great

intensity. They are at first lenticular elevations, red, violet, or greyish in colour,—rising very abruptly from the outer table, porous and spongy, sometimes hard, very seldom smooth like the normal bone. The cranial osteophytes extend little by little from their point of origin to the neighbouring parts, either preserving their original form or becoming elongated, or, indeed, assuming a crescentic shape. The frontal and parietal eminences are the last regions to be invaded, but it may happen that finally they are so, and then the entire vertex of the skull is covered by the osteophyte. The sutures themselves are united, which much modifies the development and the dimensions of the cranial cavity. The osteophytes increase in thickness, and in the most diseased parts may attain two and even three centimetres in thickness. When the diathetic influence is exhausted, the morbid tissue becomes harder and denser from the shrinking of its medullary spaces, and from the accumulation of a fresh amount of calcareous salts. In some cases the weight of the skull notably exceeds the physiological mean. The cranial osteophytes are peculiar to hereditary syphilis. They determine morphological deviations nearly always to be appreciated during life. Among its usual forms there is one which is very common and quite characteristic. It consists in the presence of two or four eminences disposed around the bregma (fontanelle), and separated by two furrows in the form of a cross; the one transverse, directed along the coronal suture, the other antero-posterior, along the sagittal suture. When these eminences are very prominent, and sufficiently extensive, a typical condition is the result. I have termed the skull thus deformed *natiform*, on account of the resemblance which its elevations present to the parts styled in Latin the *nates*. The premature union of the sutures determines the prominence of the bregmatic region, and an arrest of encephalic development, whence may result idiocy, as I have once observed.

In its spongioid period hereditary syphilis leads to still other modifications of the skull. I allude to *plagiocephalus* and *craniotabes*. Plagiocephalus cannot be produced apart from habitual decubitus on the same side of the head, but syphilis favours its development. Craniotabes also is primarily caused by prolonged dependency of one part of the skull, and it is frequently enough seen apart from syphilitic rachitis; but this latter is, without contradiction, of all the maladies of the organism, that which, combined with decubitus, provokes it in the surest and most marked manner. When both osteophytes and tabetic perforations occur at the same time on one skull, which is not rare, the two lesions always exist on points diametrically opposite. For whilst the former occupy regions of the skull which are uppermost during decubitus, the perforations, on the contrary, are found on the undermost part, that which is most directly submitted to the action of weight.

The knowledge of the cranial changes produced by hereditary syphilis has enabled me to determine two points of historical pathology. The first is that syphilis existed in certain parts of South America before the European conquest. The truth of this proposition is indisputably demonstrated by three specimens. Two belong to the museum of the Société d'Anthropologie; they are the crania of young children found by M. le Dr. Destruges at Guyaquil (Ecuador), together with objects manifestly of American origin; they have on the frontal and parietal bones osteoid osteophytes precisely resembling those which I have described. The third is an adult cranium, in the museum collection; it was found at Chancai, a few leagues to the north of Lima (Peru); the objects from the place of interment in which it was found do not, according to the opinion of most competent archæologists, show any trace of Spanish influence; and all favours the view, according to the opinion of M. Ernest Hamy, that the remains at Chancai are undoubtedly anterior to the time of Pizarro. The second historical fact is the existence of syphilis at the epoch when the Dolmen populations dwelt in the centre of France. Owing to the courtesy of M. le Dr. Pruniers, of Marvegols, to whom we owe so many interesting anthropological discoveries, I have been enabled to examine three fragments of skulls from young subjects coming from the dolmens of Cauquenos and Bourassac in La Lozère. I have ascertained in them the existence of osteophytes, wholly resembling those which are produced at the present day by hereditary syphilis, and which can leave no doubt as to the existence of the disease in those remote times.

There should remain for me to speak of the changes of the dental system; but upon this latter point I am free to confess my studies are not yet sufficiently advanced. However, they go so far as to allow me to affirm the importance and the accuracy of the observations which the illustrious President of the Pathological Society, Mr. Jonathan Hutchinson, has made on this subject.

NEW CASES OF FILARIOUS DISEASE.

BY J. BANCROFT, M.D.

I PREFACE this communication by a few additional remarks on the cases in my paper read before the Pathological Society on May 21st, 1878.

No. 2 was electrified by the magnetic machine, and made ill. Chyluria continues and excessive appetite. A mosquito that had bitten this patient contained a filaria.

No. 3. Chyluria and hæmaturia at times severe.

No. 7. Girl aged nineteen. Chyluria. Filariæ seen in blood. Chyluria ceased. Married, and in February, 1879, was delivered after hard labour by midwife. I saw her after labour; a vaginal polypus-like growth was observed, which patient had noticed for a long time. This I cut off. A blood-clot the nurse had saved I examined for filariæ, and found them abundant. The child died shortly after birth. I cut into the scalp tumour, removing about two ounces of blood. In this there were no filariæ.—N.B. Filariæ did not travel from the mother's blood to that of the child.

No. 8. Tabes; severe fever occasionally.

No. 10. Painful state of shoulder.

No. 14 died three months ago in distant town with great disorder of the chest. Surgeon who attended him had seen nothing like it before. No post-mortem was made.

No. 25. Boy, recovered from operation after severe pelvic abscess. Now the brachial, radial, and ulnar arteries have ceased to circulate blood. He had a severe febrile attack a short time ago lasting four days. No filariæ to be seen in the blood.

No. 27. Chyluria continues. Filariæ in blood.

NEW SERIES OF CASES.

No. 1. Boy, aged eighteen, farm labourer, admitted into Brisbane Hospital in 1878 with elastic tumour of upper part of right thigh. A seton was put through with no particular result. Filariæ were found in blood of the part. On re-admission in December blood of the arm examined; contained no filariæ. Tumour galvanised by Gaiffe's chloride of silver apparatus strongly daily for a fortnight; also by the electro-magnetic machine. On tumour being punctured filariæ exuded. Several ounces of blood taken from the arm contained no filariæ.—February: Soft tumour now diminished, and leaves several hard glands about size of large peas. Patient otherwise well. Hard glands not punctured to determine existence of filariæ in them. Possibly cured.

[Nos. 2 and 3 have been omitted in Dr. Bancroft's manuscript.—ED.]

No. 4. Boy, aged nineteen; labourer. Unsteadiness of hands, twelve months, worse for six months. Was ill five years ago with abscesses, chiefly on left side. Has been obliged to give up a situation in which the use of the pen was required. Can scarcely sign his name; requires very great care, and the writing looks like that of an old man of seventy. The abscesses he had had caused me to examine his blood, in which filariæ were particularly abundant. I directed this patient to catch any mosquitos that had bitten him in the night. In the first mosquito examined I counted forty-five filariæ.

No. 5. Boy, aged eighteen; farm labourer. Feb. 13th: Chyluria, with bloody admixture at times. Had abscess of right groin six years ago, abscess of left two years ago. Two attacks of pain in testicles since Christmas; they become drawn up; he then sweats, afterwards shivers for about half an hour, then has hot skin and headache, which prevents him working for two days. His hands have a peculiar numbness, which he experiences at times, and a sensation as if rain were falling on them. I notice no anæsthesia. Blood from scrotal vein contains no filariæ. Urine sets in a trembling jelly, and contains no filariæ.

No. 6 in this new series is the case of leprosy with filariæ in the blood previously sent to Dr. Cobbold, and published in THE LANCET of Feb. 1st of the present year.

Brisbane.

ON A

REMARKABLE INSTANCE OF POISONING BY MEANS OF LOZENGES CONTAINING ANTIMONY.

BY DAVID PAGE, M.D., F.I.C.,
MEDICAL OFFICER OF HEALTH FOR WESTMORELAND.

THESE notes relate to an occurrence of three cases in which alarming symptoms of poisoning followed upon eating ordinary non-pharmacopœial lozenges, but which, upon chemical examination, were discovered to be impregnated with antimony. So far as I can learn, this is the only recorded instance of the kind.

At Kendal Martinmas Fair a young girl engaged in the service of a gentleman's family there bought some confectionery at a stall in the street. It seems that she ate a couple of lozenges herself, and gave this quantity also to each of two children, aged respectively three and five years. In about a quarter of an hour afterwards all three were seized with violent sickness and retching, which lasted for upwards of an hour. In the case of the younger child a state of collapse and unconsciousness supervened, lasting upwards of forty minutes. The absence of purging was noted. Complete recovery took place a few hours later.

Mr. Walter Iliffe, F.R.C.S.E., who attended the cases, had a suspicion of poisoning by some metallic irritant, and brought the remainder of the lozenges to me. These appeared to be ordinary, colourless, and semi-opaque sugar sweets, flavoured with peppermint. Each lozenge weighed four grammes, or about sixty-two grains. Suspecting the possible presence of arsenic, I made a preliminary test, which gave no trace of this metal, but, to my surprise, yielded all the characteristic reactions of antimony. On making a quantitative estimation of the amount of antimony actually present, I obtained ·22 gr., or nearly a quarter of a grain, of the teroxide, Sb_2O_3, as the proportion in each lozenge. It is, of course, quite possible that some might contain more and others less than this, as the antimony might not have been equally distributed in the mass.

The antimony does not appear to have existed in the form of tartar emetic, but as an insoluble oxide, since, upon dialysis of the aqueous solution, it could not be detected in the dialysate; and it was only after acidifying and heating with tartaric acid that antimony passed through. In this solution sulphide of hydrogen threw down the orange-red antimonial sulphide, from which, upon conversion into antimoniuretted hydrogen, sublimates of the metal and other distinctive reactions were obtained. I cannot conceive in what way, doubtless accidental, the antimony may have got into the lozenges, which, as I have mentioned, were sold openly in the street by an itinerant vendor of confectionery.

Kendal.

A Mirror OF HOSPITAL PRACTICE, BRITISH AND FOREIGN.

Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—MORGAGNI *De Sed. et Caus. Morb.*, lib. iv. Proœmium.

CHARING-CROSS HOSPITAL.

REMOVAL OF AN ENTIRE NECROSED LATERAL MASS OF THE ETHMOID AND INFERIOR TURBINATE BONES THROUGH THE NOSTRIL; RECOVERY; REMARKS.

(Under the care of Mr. BELLAMY.)

BESIDES showing the disastrous result of strumous ozæna if not recognised and properly treated at an early stage, this case is of peculiar surgical interest as affording an example of what may be done in the way of removal of so much diseased tissue, not only without producing disfigurement, but with the effect of restoring the symmetry of the nose.

A. T——, a young woman aged twenty-one, of very strumous history, had had the symptoms of strumous ozæna for three years. Her nostril had been completely stopped up. The necrosis had probably been complete about two months. She was admitted on March 19th with great deformity of the right side of the nose. By inspection and palpation the extent of the disease could be but imperfectly made out. There was evidently a large amount of necrosed bone, and Mr. Bellamy at first thought it would be necessary to employ Rouge's operation for its removal.

On March 20th the patient was etherised, and it was then found that the disease did not extend to the palatine plate; hence it was determined to attempt removal of the mass through the nostril. This was done with great care by pulling it away piecemeal, until the nostril was evidently quite clear. The patient had no bad symptoms, and rapidly recovered; the nose quickly regained its original proportion and shape, and the voice much improved.

Mr. Bellamy remarked that in such a case as this it is important to ascertain, by means of the finger, whether the disease be limited to such portions of the osseous tissue as may be removed by the nostril, because if the floor of the nasal cavity be involved, and particularly if both sides be invaded, a more serious operation would be required. Rouge, of Lausanne, devised the method of dividing the mucous membrane of the lip, and cutting through the attachments of the lateral cartilages, lifting up the facial mass, and so getting at the choanæ—an invaluable proceeding in such a complication. Care should be taken not to use unnecessary force in withdrawing dead bone or foreign bodies from the nostril. It should also be borne in mind that in the upright position the plane of the nasal floor is horizontal, and therefore in the horizontal posture, as on an operating table, it is nearly vertical; hence all traction must be made in the direction indicated by the position of the patient. The fragment once grasped is to be steadily withdrawn. "Poking" with the forceps' point must be avoided. The nostril is very resilient, and should never be fissured. In order to be quite sure that all bone is removed, the forefinger may be easily "tucked" up behind the velum and readily opposed to the little finger introduced into the nares and parallel with its floor. As regards syringing the nasal fossæ, the nozzle used should always fit the nostril, so as to be air-tight. The syringe should be, if not a Thudichum's nasal douche, one capable of holding an abundant supply of fluid, the patient told to open the mouth, whereby the velum is closely applied to the spinal column, so that in pushing the piston gently home the fluid takes the complete circuit of the fossæ, and does not pass into the pharynx, which it would do were the mouth closed. No force should be used, and the fluid warmed. Obstinate headache occasionally follows neglect of these precautions.

BRISTOL ROYAL INFIRMARY.

CASES OF ACCIDENTAL POISONING BY ARSENIC.

(Under the care of Dr. SHAW.)

CHARLOTTE T——, aged five years, was admitted on the 13th of February, suffering evidently from the effects of some irritant poison. She was in a state of collapse, with blue and cold extremities, and almost imperceptible pulse. She had severe sickness and vomiting of brown mucus. On inquiry it was ascertained that the child, along with her mother and several other members of the family, had partaken of a "meat-pudding" for dinner shortly after one o'clock. While the mother was making this pudding, her son, a little boy of twelve years, found a paper parcel in a cupboard, containing a white powder, which he thought was flour. This he gave to his little sister, who emptied it among the flour with which her mother was making a pudding. The boy stated that he thought there would be about a teaspoonful of the powder, but very probably there was a larger quantity than this. All who partook of this pudding were seized in a very short time with pain and burning in the throat and stomach, and violent sickness and vomiting. In all, seven persons ate of it.

The first to be admitted was the child Charlotte, above referred to. She did not observe that there was anything peculiar in the taste of the pudding, but she was seized almost immediately with violent vomiting. Tenesmus, with scanty "rice-water" stools, came on shortly afterwards. As

the sickness had been so violent, no further measures were adopted for emptying the stomach. A slight saline purge was given, as well as bismuth, ice, milk, and lime-water. Attempts were made to meet the collapse by administering small quantities of brandy and applying artificial warmth. The vomiting and tenesmus, however, continued, and the child became very restless, tossing about in the bed. There was great thirst, and much abdominal pain and tenderness. She died at seven o'clock P.M., nearly six hours after having taken the poison.

Of the other four patients who were admitted to the infirmary, two of them presented very severe symptoms. One girl, aged twenty-one, had excessive vomiting, the ejected matters being of a greenish colour at first, but afterwards becoming brown. She stated that she felt a peculiar hot and bitter taste in her mouth while eating the pudding, and she was seized almost immediately with burning pains in throat and stomach. Vomiting came on in about ten minutes. She had no tenesmus, and there was some difficulty in getting her bowels to act at all. The first motion contained some slimy mucus, but differed from that of her younger sister. There was a little injection of the fauces, but no evidence of corrosive action in the mouth. The treatment adopted was very similar to that in the case of the child, with the difference that in this case morphia was combined with the bismuth. Sickness continued for about two days, gradually getting less. The tongue was very much furred on the second day. The patient had still a little pain and tenderness when discharged, but the tongue was clean, and the light diet which she was allowed did not cause her pain.

The other three cases have left the infirmary practically well. Two of them, a boy aged twelve and a young woman aged twenty-one, had pretty severe vomiting of greenish coloured matter; but the third, a young man aged nineteen, had no vomiting after admission, though it had been severe before he came in. They all presented symptoms of gastritis, lasting for about four days, and then gradually passing off. No albumen could be detected in the urine in any of the cases. With the exception of the child, the pulse in all the cases was not much quickened; it was also regular and of fair strength. The skin was cool and moist.

A post-mortem examination of the child was made about seventeen hours after death. The only pathological appearances worthy of note were found in the alimentary tract. On the posterior wall of the stomach, a little below the cardiac orifice, there was a brightly injected patch rather larger than a shilling. There were also several punctiform hæmorrhages here and there throughout the stomach, but the general injection was not great. There were two dark-coloured patches in the duodenum, one about two inches below the pylorus, and the other about the middle of the duodenum. The ileum and jejunum were healthy, but the lower part of the descending colon and upper part of the rectum were slightly reddened here and there. The stomach contained a very small quantity of partially digested food and some slimy mucus. The intestines contained a considerable quantity of rice-water fæces. The heart was almost empty.

The remaining two cases of the seven—the mother and a little girl—were treated at home, and, it is believed, have recovered well.

Chemical examination revealed a large quantity of arsenic present in the pudding. Distinct evidences of its presence were also found in the contents of the stomach and in the vomited matters.

The people could offer no explanation of how the arsenic had been obtained, or how long it had lain in the cupboard; indeed they did not know of its presence there at all. The coroner's jury returned a verdict of "Death by misadventure."

LIMERICK UNION HOSPITAL.

EXCISION OF THE ELBOW-JOINT.

(Under the care of Mr. T. O'D. RUSSELL.)

ELLEN G——, twenty years of age, was admitted to hospital suffering from disease of the elbow-joint. The disease had begun nine months before, with pain and inflammation, and was probably synovial in its origin. There was one sinus freely discharging purulent matter. The joint was quite immovable at the time of admission, and from pain and suppuration the constitution was much exhausted.

The case being considered a proper one for surgical interference, the elbow-joint was excised on Aug. 20th, the incision being a straight posterior one of about four inches in length, attempts being made to preserve as much as possible the attachment of the triceps to the fascia of the forearm. There was extensive disease of the synovial membrane, and the cartilage of the lower end of the humerus was considerably eroded, especially on the inner condyle. The cartilage of sigmoid cavity presented traces of erosion in patches, but that of the head of the radius was intact. Accordingly only enough of bone was removed to allow of the formation of a new joint, the capitular ligament especially being left intact. The extent of the disease was easily ascertained, Esmarch's bandage having been previously applied. The arm was immediately placed in an angular splint, and lint steeped in carbolic lotion (1 in 20) was constantly applied.

The case, though somewhat tedious, went on very favourably, passive motion being begun about five weeks after the operation. Professor Annandale, in THE LANCET of Feb. 22nd, advised motion to be begun as early as two weeks after operation; such advice might apply when the operation is performed under antiseptic precautions. The usual movements—pronation, supination, &c.—have been assiduously carried out up to the present time. On March 1st, she could flex and extend the joint to an angle of at least 45°, and pronation and supination were very nearly normal. She looked in perfect health.

A CASE OF PERINEAL SECTION.

James C——, fifty years of age, a thin man, with anxious, careworn countenance, was suffering from stricture of urethra complicated with scrotal fistulæ. The history of the case is briefly this. The patient had had gonorrhœa several times, the last occasion dating back about four years ago. He noticed, soon after his last attack, that he required increased exertion in making water, and that the time required in doing so was longer. As to the fistulæ, how and when they were produced he could not intelligibly state; certain he was they were subsequent to his last attack of gonorrhœa. His life for the past few years had been one of extreme misery. When he came under care he was in a wretched condition; a foul ammoniacal odour pervading the atmosphere around him. The sheets of his bed were constantly wet by the dribbling urine, his buttocks and thighs excoriated, and his scrotum about three times its natural size from chronic inflammation. Four fistulæ opened anteriorly at the root of the scrotum, one being in the mesial line. The penile portion of the urethra was utterly obliterated; not even the finest probe could gain admittance. Under these disadvantageous conditions, and after consultation with his colleagues, Mr. Russell determined to perform perineal section. On Jan. 10th the patient, under the influence of chloroform, was placed in the usual lithotomy position. At the suggestion of Dr. M. R. O'Connor, an attempt was made to reach the urethra by introducing an ordinary director through the mesial fistula. This succeeded admirably, and Mr. Russell cut down on the director through the perineum, and made it protrude through the incision. One of the sides of the urethra being now grasped with a toothed forceps, a broad director slightly curved was passed along, through the incision, in the direction of the bladder. Over this director a short silver catheter, full size, was passed, and without much difficulty introduced into the bladder. About half a pint of fetid urine at once escaped through the catheter. The instrument being tied in in the usual manner, the patient was at once put to bed. He had a good night, and next morning passed all his urine through the catheter without pain or difficulty. A mixture of henbane and buchu was ordered. Temperature 99·5°.

Each day the patient improved. On the eighth day, as there was a trace of suppuration about the catheter, it was deemed expedient to remove it. He now passed all his water through the perineum. The opening showed a tendency to close, which was obviated by occasionally passing a catheter.

At the present time no water passes through the fistulæ, which show a tendency to close. His urine, from being foul-smelling and loaded with muco-pus, is clear and of a natural odour, and he can retain it for a considerable time. He was discharged on Feb. 20th.

Medical Societies.

ROYAL MEDICAL & CHIRURGICAL SOCIETY.

MEMBRANOUS CROUP AND DIPHTHERIA.

THE ordinary meeting of this Society was held on the 13th inst., Mr. J. E. Erichsen, F.R.S., President, in the chair. The sitting, which was a very prolonged one, was occupied with the continuance of the debate upon the report of the Committee on the Relations of Croup and Diphtheria. The debate has grown in vigour on each successive occasion, and on this, its concluding evening, was shared in by a large number of speakers. Drs. Barclay, Pavy, and Andrew, and, to a certain extent, Drs. Wilks and Hilton Fagge upheld the view of the non-identity of the two affections; Sir W. Jenner, Drs. Greenfield and Dickinson, as well as Drs. Thorne Thorne and Fitzpatrick the opposite view. Dr. Wilks pointed out that Mr. Hutchinson had given a solution of the whole question by rejecting the specific nature of diphtheria, and claiming Dr. Johnson as a convert to this doctrine, he thought that there was no longer any need for dispute.

The PRESIDENT, on taking the chair, said that it was absolutely necessary, in the interests of the Society, that the debate should conclude that night. Many papers had to be read that session, and a special meeting would be held on June 3rd to enable them to be read. He expressed his willingness to sit for any reasonable time, to allow the debate to be brought to a conclusion.

Dr. BARCLAY had not contributed to the information collected by the Committee because he held it far more important for them to have facts than any theoretical conclusions based on facts, observed some time ago, and of which he had not preserved notes. He expressed his hearty approbation of the manner in which the Committee had discharged their function; and although their conclusions might be disputed, yet a great debt was due to them for the able manner in which they had treated the facts before them. These facts were, however, somewhat limited. He could not find that they embraced any consideration of the disease formerly known as "croup"—a membranous tracheitis, and not, as had been assumed, a laryngitis. Such was the teaching in Scotland when he was a student; and he maintained that in all writings on the subject the two affections were kept clearly distinct—viz., the one an inflammation of the trachea, accompanied by membranous exudation; the other a blood-change, leading to a specific membranous inflammation, which might attack the air-passages, but whose special seat was the fauces. It was this adoption of the term "membranous laryngitis" as an alternative for croup that constituted his chief objection to the report of the Committee. His own experience dated back prior to the time when diphtheria was known in England. When he was a student in Edinburgh membranous croup was well known, although it was somewhat rare. But at present it becomes a question whether membranous croup is ever seen at all. Certainly he could not find among the cases given in the report any corresponding to the disease as he had known it, or as it had been described by authors on the subject. From Edinburgh he went to Paris, and found "diphtherite" to prevail there, where it was thought to be the same disease as English croup; and he remembered the surprise he felt at finding Guersant at the Hôpital des Enfans Malades holding this "diphtherite" to be so infectious that he dared not apply a blister to a healthy child if another suffering from that malady were in the same ward, for fear that false membrane would form on the raw surface. No doubt this propagation of the disease was due to the extremely insanitary state of the wards in the Paris hospitals in those days; but there could be no doubt of the contagiousness of diphtherite, which, moreover, often attacked the vulvæ of children in the same ward as the pharyngeal cases. Then returning to England he saw again cases of membranous croup (certainly but few in London, they being, he thought, more prevalent in the country), but they never presented the high degree of contagiousness which he had seen in Paris. Then diphtheria, known at first as the "Boulogne sore-throat," was imported into England, and we saw cases of that disease in which three, four, or five children might be attacked in a family and die right off; quite lately one such instance had occurred in his practice. Such was perfectly unknown in his student's days in Scotland. It might be that, as with scarlet fever, there were two forms of the disease—the one mild, sporadic, endemic; the other occurring in an epidemic form and of great severity. We do not know the conditions giving rise to an epidemic of scarlet fever, nor why it varies with the seasons, but we know that it will sweep over the whole metropolis, and after a time will come to an end. But all the while sporadic cases continue to occur from week to week, and day by day, as before. May not the same be true of diphtheria? and, if so, it is quite conceivable that croup of olden days was sporadic diphtheria. At one time albuminuria was thought to be the test of diphtheria; but this was now known not to be the case, albumen occurring in other forms of throat affection. He was quite prepared to admit that in the larger number of cases of croup albuminuria was absent, and that it was present in the majority of cases of diphtheria; but this was not enough, nor could the non-epidemic character of croup, contrasted with the epidemic occurrence of diphtheria, help in the distinction. In seeking for a distinction he felt inclined himself to hark back to the old definition of croup, which implied its limitation to the trachea. Were this not so he would give up the point, and declare in favour of identity. But he recalled cases of membranous croup in children who still could speak clearly and distinctly, but who died asphyxiated from membrane driven against the glottis from below. In some such cases membrane had been expectorated whilst the voice was still retained, showing that the larynx could not be involved. He remembered one case in which emetics failed to cause ejection of any membrane, nor was any found after death, only an inflammation of the trachea ceasing at the lower part of the larynx, where scanty puriform secretion was found. This was a case of fatal non-membranous croup. Dr. Barclay concluded by stating that the Registrar-General's return did not admit of estimation of cases of diphtheria.

Dr. HERON, referring to the question of cold as a cause of membranous croup, criticised the remark of Dr. Bowles, that under "cold" were included atmospheric and telluric conditions as well as low temperature. The reports of the Registrar-General were opposed to the view of cold being an exciting cause of croup. Thus, in 1875, marked by much cold weather in all the seasons, the mortality from croup, instead of being large, was the lowest but four of any of the preceding sixteen years. The exceptionally prolonged winter we had just passed through, with an average temperature of 39·4°, yielded a smaller death-rate from croup than the average of the preceding ten winters; the mortality from October, 1878, to March, 1879, being 338, and the average referred to being 357; the mean temperature for these ten winters being 42·3°. The difference would be even more striking were allowance made for the increase of population. Such facts showed that cold was not a direct cause of croup.

Sir W. JENNER had no facts to add to those adduced in the report. He would preface what he had to say by reading a short sentence from the lecture he gave in January, 1875, which had been often directly and indirectly referred to, sometimes, he imagined, by many who had never read what he had written. The passage was: "My opinion has undergone some modification, and I am inclined now to the belief that there is no such disease as idiopathic, simple, membranous inflammation of the larynx. I say, I am inclined to this belief. I am not sure that it is true, but as I formerly thought that the weight of evidence was in favour of their non-identity, I am now inclined from my further experience to think that the two diseases are really identical, that the so-called croup is really diphtheria." Thus, in 1875, he had said that his experience led him to incline more to the view of their identity—not to declare it absolutely, for he had not the grounds for basing so certain a statement upon the unity of croup and diphtheria as, for example, there existed in distinguishing measles from scarlet fever. He looked to the report in the hope, but not with the expectation, that it would solve what little doubt there was left in his mind. He did not expect it would do this, for he thought that the circular of questions sent round to the profession was little

likely to elicit truth. He thought that men's opinions would colour the facts they narrated, so that these facts would come to be only the mere reflex of opinions personal to the individual, or derived by him from his teachers. That suspicion was fully verified, and the committee had acted wisely in coming to no conclusion, for they had no facts upon which to come to a conclusion. And so it happened that what conclusions they did give could be read in two ways. Mr. Parker could find no conclusions; Mr. Holmes found nothing could be more definite than the conclusions given. Such differences reminded him of the endless disputes about Hamlet, whether he were mad or not. The commitee had not the data for final conclusions. Their able report was a clear reflex of prevalent views, and would prove a starting-point for further research and future opinion. He was told that croup and diphtheria could not be identical because there was a sort of universal feeling in the profession that they were two distinct diseases. The mere strength or universality of an opinion did not prove its truth. There was a time when the whole nation believed in the existence of witches, nor should it be forgotten that in our own profession it was once generally held that measles and scarlet fever were identical. But he did not find this general agreement; on the contrary, there was the greatest diversity of opinion. He was surprised to hear Dr. Barclay say that in croup the false membrane was limited to the trachea, so that if this were found in the larynx the case would be one of diphtheria. He would ask Dr. Barclay if in any post-mortem examination made in the last thirty years he had ever found membrane limited to the trachea. He would ask any who were in the habit of making such examinations if that was ever their experience, and he quoted Dr. West in support of this statement. Then some made the distinction that the one disease, croup, is sthenic; the other, diphtheria, asthenic. This was so baseless and void of anything tangible (for some cases of diphtheria were sthenic, and membranous laryngitis was both sthenic and asthenic) that he could find no resting point here for his foot. Then the greatest differences prevailed as to the frequency of this—e.g., at one hospital (the North-Eastern Hospital for Children), of two physicians, one says that he sees croup constantly; the other, that he never sees croup, only diphtheria. Then, turning to the report to see what evidence there was of special differences in the membrane, he finds an Edinburgh physician asserting that in diphtheria the false membrane spreads upwards, in croup downwards; and that in diphtheria it can be removed by Condy's fluid! He then went to the statements of men who had specially studied the subject. He found Dr. Squire asserting that croupous membrane was distinctive in its chemical and physiological characters. If this were the fact, it would be most important, but he found in the same report one of the pathologists who was most thoroughly versed in the knowledge of the lesions of the two diseases, one of the soundest and best informed of pathologists—he meant Dr. Wilks—he found him stating that after careful and long consideration he could establish no anatomical difference between the diphtheritic and the croupous exudation. So here again no firm distinctions could be found; and turning to the clinical evidence, he found Dr. Hilton Fagge stating, in reference to his tables of cases, that in Class 2—cases of primary membranous laryngitis—"we have a disease which seems not in a single instance to have arisen in contagion, nor to have spread to other patients"; but in reference to the table, we find that in case No. 74 in this class there was scarlatina in the house five weeks before the child caught cold, and that the child's sister had been in the hospital with croup, for which tracheotomy had been performed about three weeks before the other child was taken ill, and that she had been sent home only five days after the operation. Contagion could not be excluded in that case. In Dr. Dickinson's remarks to his table, the case of an infant is related, who was tracheotomised for membranous laryngitis, and from whom the nurse contracted diphtheria. There were two reasons why those cases of membranous laryngitis should be less contagious than the pharyngeal form of diphtheria. There was the great difficulty in coughing, and at any rate of scattering the membrane, after the trachea was opened. Then, when the child has diphtheria in its early stage it is running about. A notable instance of this occurred in the lamentable affair at Darmstadt. The child goes about, kissing his brothers and sisters, but as soon as the membrane attacks the larynx he is isolated and placed away. So that there is ess liability and opportunity for contagion to take place from the laryngeal cases. As to albuminuria, Sir William noticed that Dr. Fagge states that in Class A, "in only one of these nineteen cases was albuminuria noticed to have been present." But these nineteen cases extended over twenty-one years; the albumen might have been overlooked. A blind man might say that there were no people in a room which was full of them. Dr. Gee's cases of a similar kind gave a considerable proportion in which albuminuria was present, and in one of them the albumen did not appear until six days after the child was admitted, and many of Dr. Fagge's cases died before that time. But the same criticism would apply to the absence of false membrane on the pharynx. He did not suppose that clinical clerks were different at Guy's from those elsewhere, and when it was remembered how difficult it was to examine the throat of a child thoroughly, it was almost impossible that this could be done by a clinical clerk. The value of enlargement of the glands had been greatly over-estimated. In the pharynx the mass of absorbents go to the superficial glands, which readily enlarge on inflammation about the fauces; whereas the absorbents from the larynx go to the deep glands, glands which, small in size and deeply seated beneath the fascia, showed no visible external signs of their involvement. After death such glands were found enlarged, which could scarcely be felt during life. Therefore, neither as regards the presence of the false membrane, nor the presence of albuminuria, nor the relative contagious nature (if the larynx be involved death may occur in a few hours and allow no time for contagion), nor the involvement of the glands, could he find any firm footing to establish a distinction between the two diseases. Nor was he alone in that, for he found that Dr. Wilks himself says that he cannot distinguish them. Dr. Pye-Smith gave grounds similar to those of Dr. Fagge for separating them. So that, whether the general opinion of the profession or the special opinion of those who had studied the subject be regarded, he could find no resting-place for his foot, except that ground which assumed that both affections were one and the same.

Dr. WILKS had not intended to speak; but the whole complexion of the subject had been so altered by the speeches of Mr. Hutchinson and Dr. Johnson that he could not refrain from making some remarks. Having stated the reasons why the committee was appointed, he said that for many years Dr. Semple had urged that membranous croup was a form of diphtheria; and Dr. Wilks here added that he perfectly agreed with Sir W. Jenner in denying the existence of a membranous tracheitis apart from a laryngitis. Such a view dated from pre-pathological times, and was based on the expectoration of material which resembled membrane. The question before the committee was very definite. It lay in a nutshell, and in answering it he gave it in his own words, "Is there such a thing as an idiopathic membranous inflammation of the air-passages?" But according to some it seems this question should not have been answered; the committee should have stated what they meant by specific diphtheria. He had always understood a specific disease to be a disease like one of the exanthemata, contagious and protective—e.g., small-pox. One of the methods of testing the specificity of small-pox would be its clinical history. Mr. Hutchinson had made a remarkable and conclusive speech, and he (Dr. Wilks) was much inclined to go with him. Mr. Hutchinson did not regard diphtheria as specific, and spoke of epidemic sore-throat originating in diphtheria. Hitherto Dr. Wilks had thought that Dr. Johnson was one of the great champions for the specific nature of diphtheria, but he found him quoting case after case in which "diphtheritic" affections arose *de novo* from bad drainage, and surely that was then an idiopathic disease. So that it seemed that Dr. Semple was left all alone to fight out the specificity of diphtheria. So we came to this, that diphtheria and membranous laryngitis were not specific diseases—that is to say, they were as idiopathic in their origin as cirrhosis of the liver in a gin-drinker, or granular kidney in a port-wine bibber. In such a sense diphtheria was not specific, and although Sir W. Jenner would not hold this, he yet thought this the logical outcome of the arguments in favour of the *de novo* origin of diphtheria that had been advanced by Dr. Johnson. Then, are the circumstances producing diphtheria the same as those producing membranous laryngitis? All agree with Mr. Hutchinson in the fact of the occurrence of epidemic sore-throat, and there was no reason for dissociating membranous laryngitis from diphtheria in the same sense. So that he felt that all the time he had been fighting and

imagining a giant, and fancying that Dr. Johnson was opposed to him, he found suddenly that, like the prophet of old, he had given blessings where curses had been expected. But he differed from Dr. Semple as to the use of the word "croup." He held it to be a very convenient term, used by all in a very definite sense. He had never been called to a case which was not "croup." It might not be possible to show the presence of false membrane; the child might die, and, no post-mortem examination being obtained, it would never be known if false membrane were present or not. But the case would be one of croup—a disease of sudden onset with, in a few days, implication of the larynx. It was a correct term, and should be retained.

(To be concluded.)

PATHOLOGICAL SOCIETY OF LONDON.

The Osseous Lesions of Hereditary Syphilis.—The Occurrence of Organisms under Antiseptic Dressings.

THE ordinary meeting of this Society was held on the 6th inst., the President, Mr. J. Hutchinson, in the chair. The proceedings, which were prolonged to a late hour, were of considerable interest. In the first place, M. Parrot, of Paris, exhibited a large collection of bones showing the lesions he has described as peculiar to hereditary syphilis. His address on the subject, which was delivered in French, was remarkable for its lucidity and simplicity, and was frequently applauded. Drs. Barlow and Lees exhibited children showing the cranial changes referred to by M. Parrot, and Mr. Macnamara urged that rickets, and not syphilis, was the cause of the changes. To this M. Parrot replied by stating that the latter disease frequently induced rachitic changes. Then followed a paper by Mr. W. Cheyne, describing a series of experiments demonstrating the occurrence of organisms under antiseptic dressings. Mr. Lister made some observations upon this subject.

On the President's invitation M. PARROT rose and described the Osseous Lesions met with in Hereditary Syphilis. The text of his address, rendered into English, will be found on another page.

The PRESIDENT, on behalf of the Society, returned special thanks to M. Parrot for the interesting and graphic manner in which he had described to them the features of his contribution to pathology. He reminded the members that a few weeks ago Dr. Barlow had shown some skulls presenting the conditions described, and that M. Parrot had established the fact that these conditions, which had been thought to be rare and almost accidental, were, indeed, frequent and constant, and of very early occurrence in infantile life. This resulted from M. Parrot's zeal in examining large numbers of cases, even those which during life showed no marked change. He had also been enabled to formulate rules as to the localities attacked by these lesions. He had thus localised the regions of the skull involved, and had added one very important sign whereby hereditary syphilis may be established long after infantile life—viz., the existence of a depression between the parietal bones giving the "natiform" variety of skull.—Dr. LEES said that all must agree as to their indebtedness to M. Parrot for his lucid demonstration of these changes. Syphilis in connexion with the osseous system was not well known apart from inflammation of the epiphyses, and only a few weeks ago an article in the *Centralblatt für Kinderkrankheiten* entirely ignored the cranial changes, whilst dwelling at length on the epiphysial. He had brought two children to the Society who showed well the deformities described by M. Parrot. One was eight or nine months old. At three months it presented well-marked signs of congenital syphilis, getting better after mercurial inunctions. When seven months and a half old four rounded bony swellings appeared on the head, occupying the anterior and superior angles of the parietal and the posterior superior of the frontal bones. There was also thickening along the sutures to the same extent. The parietal eminences were not affected. Posteriorly there is the condition of "craniotabes"; the bone is as thin as parchment. This condition was not so common in pure rickets as had been supposed; most examples of it that Dr. Lees has seen being in syphilitic children. The other child was two years and four months old, in which the cranial changes were more marked and general. In the frontal region was a large rounded swelling, at one part having a groove for the reception of a vein. The child's symptoms commenced with snuffles and a rash at three months, epiphysitis first of one elbow, then of another, then of knees; at twelve months inflammation of both tibiæ; and now these bones were massively thickened along their upper two-thirds. The child was only slightly rickety, and the syphilitic history was very marked.—Dr. BARLOW showed a third case, a child eight or nine years old, showing the later changes described by M. Parrot in a very marked degree, the enormous prominence of frontals and parietals and "natiform" character of skull. The child also had interstitial keratitis and pegged incisors.—Mr. MACNAMARA thought that there might be some tendency to confound rickets with the changes described by M. Parrot. The youngest of the children exhibited showed marked rachitic signs—craniotabes, beading and curvature of ribs. He could not think craniotabes the result of syphilis; and as to the other changes—e.g., between the diaphysis and epiphysis,—there was a marked distinction between the cell-proliferation of rickets and the degenerating inflammatory products of syphilis. It was not at all improbable that rickets should complicate syphilis, since it arose from malnutrition. He inquired if the cases showing the changes in a marked manner had been treated with mercurials.—M. PARROT, in reply, said that he had pointed out that craniotabes resulted from the combined effect of pressure and general malnutrition, and among these latter there is none more likely to be associated with it than hereditary syphilis, although he had seen it in children who were not syphilitic. He was not able to regard rickets as a peculiar disease. In syphilis there is a rachitic period when typical changes of malnutrition occur, and among these are the changes which ordinarily occur in rickets.

Mr. WATSON CHEYNE then read a paper upon the Occurence of Organisms under Antiseptic Dressings. He began by referring to some observations made five years ago by Ranke, of Halle, who found organisms present in the discharge under antiseptic dressings in nearly every case he examined. His observations were, however, only made with low microscopical powers, and had been regarded by many as faulty. Since then similar statements had been made by Demarquay, Fischer, and others, but in all the objection was that the antiseptic processes had not been thoroughly carried out. When Mr. Cheyne became house-surgeon to Mr. Lister, in 1876, he commenced an inquiry into this subject, which he has pursued to the present time. He had never found rod-shaped bacteria in the discharge from wounds treated antiseptically. This discharge containing hardly any leucocytes, and these, if present, being quite granular, and also containing much granular matter and fibrin, renders it very difficult to determine the presence of micrococci. So that he had recourse to "inoculation experiments," using for the purpose infusions of cucumber, precautions being taken to boil the fluid for twenty minutes, and to decant it under carbolic spray into small purified flasks. These flasks were "inoculated" with a drop of the discharge taken up from under the dressing in capillary vaccination tubes, and the fluid thus inoculated kept at the temperature of the body in an incubator. The capillary tube was always previously purified in carbolic lotion, then heated to dryness in a spirit-lamp, rapidly introduced into the drainage-tube, and thence into the flask, which was opened under the carbolic spray. When development of organisms took place the fluid became turbid in from thirty to fifty hours. He found that the fluid inoculated with discharge from cases kept antiseptic either remained perfectly clear or else became turbid from the development of micrococci. But in either case the patient showed no local or general disturbance. In cases *not* treated strictly according to antiseptic principles *bacteria* always occurred in the inoculated fluids as well as micrococci. In many of such cases the antiseptics had been employed externally and also injected into the wounds, but no precaution had been taken either to penetrate all the recesses of the wound with the injection or to prevent the access of organisms during or after the dressing. In few cases originally treated antiseptically bacteria were found, but in all their presence was indicated by a disagreeable smell, or by symptoms of local or constitutional dis-

turbance. One was a case of psoas abscess in a weakly man; symptoms of carbolic-acid poisoning appearing, the dressings were made smaller, and were left on longer than usual, on some occasions for thirty-six hours after the discharge had appeared at the edges. The appearance of bacteria in such a case was not surprising; and the foul odour of the discharge, profuse suppuration, hectic fever, and finally death, proved the inadequacy of the dressings in this case. Cases such as this prove that bacteria, as well as micrococci, can flourish under antiseptic dresssings. Mr. Cheyne believed with Cohn and Rindfleisch, contrary to the views of Billroth, Hallier, and Klebs, that micrococci are *not* bacteria altered by their environment; and he has obtained decided evidence of their being distinct forms. Micrococci are colourless or coloured round cells of very small size, with or without movement, growing from pairs into short chains or groups of larger or smaller size, not derived from bacteria nor growing into them. They grow luxuriantly in cucumber or turnip infusions. In milk they produce but little change as regards smell or taste, but the milk separates into two layers, an upper clear, and a lower white granulated deposit. This absence of active properties in producing but little change in cultivating liquids affords an answer to the question, why the cases of wounds in which they are present do not differ apparently from those from which all organisms are absent, for these organisms, when they grow in wounds, produce no smell in the discharge, no local irritation, nor are they poisonous when introduced into the circulation. In these negative qualities they contrast markedly with the ordinary forms of bacteria. In none of the cases of septic disease in which the presence of micrococci has been laid stress upon have there been sufficient evidence to show that bacteria are absent; such diseases included diphtheria (Oertel), erysipelas (Recklinghausen, Lukomsky), ulcerative endocarditis (Hieberg, Eberth), unhealthy wounds (Birch-Hirschfeld). Professor Gerald Yeo, at Mr. Cheyne's request, found that cucumber infusion containing micrococci, injected into the jugular vein of a rabbit, produced no effect; whereas an injection of fluid containing bacteria led to the death of the animal on the fourth day. Many such experiments were performed, with the invariable fatal result following injection of a bacterogenous fluid, but never that which only contained micrococci; and like differences occurred in injection into the subcutaneous cellular tissue or the cornea. No doubt many forms of micrococci exist, and Koch, in his recent work, adduces strong evidence that pyæmia in the rabbit is due to the development of micrococci in the blood. But it is quite certain that if there are noxious forms of micrococci, they are rare, and that the conditions which allow of the entrance of ordinary forms to wounds treated antiseptically do not admit of the entrance of hurtful kinds. Still, there remained the question, Why do micrococci and not bacteria gain access to wounds treated antiseptically? How, indeed, is it that organisms of any kind are found in the discharge from aseptic cases? They may enter the blood, and thus reach the wound; or may enter through some loophole in the antiseptic method or its faulty application; or, lastly, they may arise by spontaneous generation. Each of these possible modes of entrance Mr. Cheyne proceeded to discuss. As to the normal occurrence of organisms in the blood or tissues of the healthy living body, he cited the experiments of Tiegel and Billroth, showing that organisms developed in the inferior of organs taken from a newly-killed animal and dropped into melted paraffin, experiments which Dr. Sanderson quoted in his Brown lectures. Mr. Cheyne, repeating these experiments on a different method, obtained negative results. He placed the organs from animals killed under carbolic spray into beakers of cucumber infusion, and these then kept in the incubator were used to inoculate other flasks of the infusion. No organisms were developed, although the fluid was most favourable, being not only cucumber infusion, but also an infusion of meat. If micrococci were injected into a rabbit, its organs removed and examined in the above manner always yielded micrococci; if bacteria, then those appeared in the infusions containing the organs and in the fluids inoculated from them. But the absence of organisms from the blood has been proved by the experiments of Klebs, Lister, and Sanderson. Then Lister and Pasteur have shown that urine is sterile; and the former has shown this to be true also as regards milk. Koch denies the existence of organisms in healthy animals; and Chiene and Ewart coincide in this. In Tiegel's experiments there are many fallacies; for the organisms might have gained entrance into the organ, though the divided bloodvessels, in its transference from the animal to the paraffin. But in states of disease there are certain conditions under which organisms may be present in a state capable of development in the blood and tissues. Thus Sanderson has shown that organisms may be found in a healthy animal in whom an acute inflammatory process has been induced by the subcutaneous injection of ammonia. In profound nutrition changes, as those induced by slow phosphorus poisoning, organisms may be found in the blood and tissues. But healthy blood will destroy organisms. Thus, in acute inflammatory processes, or in great interference with the nutrition of an animal, organisms may pass into the blood without losing their vitality. The organisms found are usually micrococci. Mr. Cheyne had found that, whereas chronic abscesses yielded no organisms, in acute abscesses seven out of thirty-two inoculations yielded micrococci, never bacteria—results similar to those obtained by Billroth. Godlee and others have shown the existence of organisms in the medullary canal in osteomyelitis, generally micrococci, bacteria being found only in the most severe cases. From this it appears that only in conditions of considerable inflammation or depression of nutritive processes can organisms reach the wounds through the body generally. After antiseptic operations there is very little disturbance, and the organisms do not appear in the discharge until after some days have elapsed; so that their entrance from the blood, although not impossible, is not the ordinary way. Is their entrance due to loopholes in the antiseptic treatment? The lotion and the spray are both quite potent to destroy organisms. Of this he had ample proof. There remained the dressing itself, and as it is at a late period in the treatment that organisms appeared, it is likely that here, if anywhere, the loophole occurs. In some cases a daily change of dressings will prevent the development of organisms, but if the dressings be left on longer than usual, micrococci are sure to gain entrance. When the discharge is profuse the carbolic acid in the dressing is not in sufficient quantity to prevent the development of organisms, and as the carbolic acid is very volatile, much will have escaped when the dressing is left on long. Moreover, carbolic acid added to albuminous fluids (e.g., milk or serum) causes a precipitate, and requires much more of the acid to prevent the growth of organisms in these fluids than it does in Pasteur's fluid. Then came the question, Why do only micrococci, and not bacteria, develop in antiseptic wounds? The simplest explanation seemed to be that the micrococci could grow in fluids containing carbolic acid in larger quantity than those in which bacteria can develop. In pure cucumber infusion bacteria will develop in from twelve to twenty hours after inoculation; micrococci in from thirty to fifty hours. But if to the fluid a solution of carbolic acid (1 in 500) be added, the result is the reverse. Moreover, if to the carbolised infusion fluid containing micrococci and bacteria be added, the former develop to the exclusion of the latter; whereas, if the same be done with the pure infusion, the bacteria develop to the exclusion of the micrococci. That this is not because in the one condition the bacteria grow as micrococci is evident, since no micrococci develop in fluid which contains bacteria. [This he had several times proved in his experiments; for, the air of the room being charged with bacteria, he could never succeed in "cultivating" micrococci in fluids without the use of the spray.] Tap-water added to cucumber infusion is followed by the development of bacteria; if to carbolised infusion, by micrococci. The largest proportion of carbolic acid in cucumber infusion in which organisms can develop is from 1 in 450 or 1 in 500; and in experimenting to find whether micrococci developed in fluid containing a larger proportion of carbolic acid than did bacteria, he found that by successive inoculations from infusions containing carbolic acid in varying degrees of dilution micrococci *could* be made to grow in a dilution of 1 in 300, each successive inoculation being made from the mixture containing most carbolic acid. The conclusion arrived at from such experimentation was that the discharge, when profuse, or when it arrives at the edge of the dressing—this, which has been left on for some days, does not contain enough carbolic acid to prevent the development of organisms. That micrococci, which abound far more than bacteria do in the ward atmosphere, find this a favourable medium for growth; that as they grow they increase in vigour and are able to live in fluid containing a larger proportion of carbolic acid; and that if time be given, they

will ultimately reach the wound. As to the possibility of spontaneous generation, this, Mr. Cheyne pointed out, had been so disproved of late that it was almost needless to discuss it. Were it frequent, such methods of investigation as those he had employed would be impossible, for under various conditions of atmosphere, temperature, &c., organisms would develop spontaneously in the cultivating fluids; and yet he has kept some flasks of cucumber infusion or of milk for months, until they have dried up, without in any instance organisms developing, unless previously introduced. But if they can arise spontaneously in a wound, why do not bacteria thus occur? If their origin be due to external conditions, why do they not occur in all wounds in the same ward at the same time? or if their appearance depended solely on some change in the nutritive or other functions, why do they not occur in all the wounds of the same patient at the same time? The general conclusions were, that when the antiseptic treatment is thorough, organisms are either completely absent from wounds, or, if present, they belong to the class of micrococci; whilst in wounds *not* kept under antiseptic precautions, bacteria develop. That bacteria can live under antiseptics when they gain an entrance. That while micrococci give little indication of their presence, bacteria are indicated by the odour of the discharges and the local and general disturbance of the patient. That micrococci are distinct from bacteria in their mode of growth, in their relation to reagents and carbolic acid, in their effects on fluids and on the living body, and by the fact that they have never been observed to pass one into the other. That the ordinary forms of micrococci are harmless in the body, contrasting markedly with the bacteria, which are so harmful. That organisms do not occur in healthy living blood or tissues, though they may be present in states of disease, as in acute inflammatory processes or in great depression of vitality. That they are not essential to the inflammatory process, which may, however, be complicated by their presence. That the explanation of their presence in wounds is that the discharge flowing from underneath an antiseptic dressing is not too strongly antiseptic to prevent the development of organisms. That organisms which find it the most suitable pabulum are micrococci, which by growing in it become stronger and abler to grow in fluids containing more carbolic acid. That this is the ordinary mode of entrance to wounds, though they may possibly in some cases come from the blood. Finally, that the entrance of bacteria through dressings is the result of carelessness, and their entrance from the body can only happen in the most grave disturbances of the vital functions, in such conditions that only operations of the utmost necessity will be performed.—The PRESIDENT said that this was a most important contribution to one of the most magnificent discoveries of the day, and he called on Mr. Lister for some observations.—Mr. LISTER said that he had nothing to add. Mr. Cheyne had demonstrated clearly what he himself had always felt was the case, the efficacy of the spray in the antiseptic treatment; for, following out methods of experimentation similar to those which he (Mr. Lister) had the honour of bringing before the Society fifteen months ago, Mr. Cheyne had rendered these methods more rigorous by conducting them under the spray. Further, he always took care to perform, side by side with his inoculation experiment, a control experiment, using the same fluid, but not inoculating it with discharge, and in every case the flasks of the "control experiment" remained barren. The conclusion that organisms of any kind could exist under antiseptic dressings was an unexpected one to him (Mr. Lister), for the evidence hitherto adduced in favour of this by some German observers has been very inadequate. To be sure, antiseptics may be used and yet wounds may putrefy; that was common enough from imperfect application of antiseptics. Mr. Cheyne had shown that the ordinary septic bacteria never occur unless the wounds are putrid, but he had found micrococci (of which, as Koch says, there may be many forms) in antiseptic conditions, the micrococci in question being able to resist carbolic acid more than bacteria can; a parallel difference in resistance to reagents Mr. Lister had found with some fungi in the presence of boracic acid. In Pasteur's solution to which boracic acid was added, only two common forms of fungus could thrive. Pasteur had long shown this difference in the resistance of organisms. One of his experiments consisted in passing milk through red-hot platinum tubes. Bacteria developed in the milk, but not the "B. lactis," for the milk did not sour, showing that that particular form of bacterium was destroyed by a temperature which would not kill the other forms. Other points made out by Mr. Cheyne harmonised with daily experience; but it suggested that some more perfect means might be found whereby *all* organisms could be excluded. One important observation was that proving that bacteria do *not* exist in the organs of healthy animals. When Mr. Lister first heard of observations to the contrary he was staggered, for they were opposed to the fundamental facts of antiseptic surgery, and he conceived that there were flaws in the experiments of Tiegel and others. They forgot that the knife used to cut out the organs, although red-hot at first, would cool, so that the whole organ would not be heated, nor were any antiseptic precautions taken; but the excised piece was placed in melted paraffin (which melts at a temperature far below that necessary to ki organisms), with no security against the entrance of air through the divided bloodvessels. Mr. Cheyne had completely disproved the truth of these observations. The observation that conditions of malnutrition favoured the development of organisms (e.g., in chronic phosphorus poisoning the organs did contain organisms) was in confirmation of M. Pasteur's and his own statement, some years ago, tha healthy tissues can resist the development of organisms and as an instance of this he cited the occurrence of fungi on a fern enfeebled by cold, the fungi being cast off as the fern recovered itself and developed more vigorous fronds.

Dr. NORMAN MOORE having exhibited a recent specimen of Abscess of the Liver, the Society adjourned.

OBSTETRICAL SOCIETY OF LONDON.

THE meeting of the Society on May 7th (Dr. Playfair, President, in the chair), was occupied with the discussion

ON THE USE OF THE FORCEPS, AND ITS ALTERNATIVES IN LINGERING LABOUR.

After the reading of Dr. Barnes' paper, which will be found *in extenso* elsewhere,

The President said:—I am sure, gentlemen, I shall be expressing the unanimous opinion of the Society if I state how deeply indebted we are to Dr. Barnes for his very interesting and philosophical paper. It is one of the most valuable contributions on the use of forceps with which I am acquainted. I shall not now trespass upon the time of the Society by making any observations, inasmuch as there are several gentlemen present who have come from a great distance, and whose views on the subject of discussion we shall all be anxious to hear. I may just say that the one point on which Dr. Barnes did not seem to me to enter so largely as the terms of the discussion might have admitted, and upon which we shall be pleased to hear the experience of any gentleman who will enter into it, is the value of uterine compression and ergot as taking the place of the forceps.

Dr. GEORGE KIDD, of Dublin, being called upon by the President, said: I would rather that some of your London leaders had risen to open this discussion. I came here far more to obtain information than to take any part in the proceedings. The subject is one which has, however, occupied a great deal of my attention, and a great deal of the attention of the Dublin Obstetrical Society, for some time past. With regard to the use of the forceps in what may be called the low operation, I believe we are all agreed that it is advisable—that it contributes to the safety of both the mother and the child. Some eight or ten years ago, when I first had the honour of sitting as President of the Dublin Obstetrical Society, I laid before the Society an analysis of the published reports of the Rotunda Lying-in Hospital, in which I attempted to show, and I still believe I did show, that in proportion as the forceps were freely used in cases of difficult and tedious labours the deaths of the mothers decreased, and the lives of the children were saved. I scarcely think it necessary to occupy the time of the Society by dwelling upon this point. The more important point I think is with reference to the high operation, as it

has been called. Now Dr. Barnes has spoken to-night of the high operation, and I think he has not clearly defined it. The proposition that has been made of late days is that we may use the forceps before the os uteri is dilated. Now, as the term "high operation" has been generally accepted as applying to where the os is dilated, but the head is still lying at the brim, on that point there can be little difference of opinion. If the os is fully, or nearly fully, dilated, and the head lying at the brim, and making no progress, I do not think any of us will hesitate to apply the forceps in that case. But when a gentleman tells us that with an os that will barely admit the top of his finger he would proceed then to apply the forceps, he throws down a challenge which I for my part cannot resist. The important and essential point, I believe, is this: Are we to use the forceps where there is an undilated os uteri? Dr. Johnston, who is the great advocate for the early use of the forceps, and the use of it in the first stage, lays it down as a rule that it may be used with an undilated os uteri, but it must be a dilatable os. I believe that there are many of these cases in which it is quite unnecessary to use the forceps, and that there are many in which some of the alternatives will be a great deal safer for both mother and child. You may have an undilated os, but in a dilatable condition, under varying circumstances. In the first place, it is not uncommon to meet with the os in this condition when the membranes have not ruptured. I have seen cases were a patient has been many hours in labour and the os has not yielded; the cervix is full, the membranes are unbroken, but they do not come down so as to press upon the os and dilate it. The os may be not larger than will admit the tip of the finger. But, Sir, if one were to introduce the finger into that os and detach the membranes around the cervix, wait a little time, and when a uterine pain comes on make a little gentle pressure with the pulpy part of the finger against the margin of the os, that os will dilate, and the membranes will soon come down and form a wedge-like protrusion through the os and dilate it sufficiently, and the patient will deliver herself in a short time without any interference of instruments. I believe that is a much safer and better practice for both mother and child. Then, Sir, you have the os undilated, but in a dilatable condition, after the membranes have broken. The membranes may break early, and the os dilate. The head comes down upon it and takes the place of the membranes, and in a little time will dilate that os. There may be delay about it, it may be a slow process, it will be a painful process, more painful to the patient than if the membranes were unbroken, but it is a process which will be accomplished in a short time, and which the obstetrician may greatly assist without the use of the forceps. If he pass his finger into that os and sweep it gradually and cautiously round, it will yield very quickly before him. If the patient has been properly treated that os will yield in a short time with very little assistance; but if she has been over-stimulated and over-heated, and encouraged to make voluntary exertions, and the head is forced violently down upon it, she may become feverish, and the parts become inflamed, and you will have a quick pulse and a great deal of restlessness and pain. Still, I do not believe it is a condition in which it is wise for us to introduce the forceps. If we have an os uteri dilated two-fifths, as Dr. Johnston has proposed to measure it by fifths, or to the extent of about an inch and a half in diameter, to proceed in that condition to introduce the forceps, to drag the head down through it, is, I believe, a dangerous practice—a practice very likely to lacerate the uterus, and to be followed by evil consequences to the mother. Whereas by making use of some of the alternatives—letting the patient have a warm bath, douching her with warm water, giving her a good dose of chloral, or putting her under the influence of chloroform—in a few hours, or less than a few hours perhaps, the os will dilate, the parts will become soft and yielding, and the head will come down through it, and then, if necessary, you can employ your forceps with safety and with advantage. But there is a class of cases in which the head remains above the brim and the os is not fully dilated, in which I am sure to apply forceps is a good practice and a very desirable one; where there is some minor disproportion, where you have a malposition, where you have an occipito-posterior presentation of the head, and the head does not come down upon the os to dilate it. The membranes have dilated it so far that at length they have given way, and the os remains with no pressure upon it, because the head cannot come down to press upon it. If you leave a patient to the throes of labour unaided and unassisted, you will soon have powerless labour set in; the uterus will become inflamed, the woman will become feverish, and you endanger her life. In a case of that kind I quite go with the proposition that to use the forceps is a useful and most desirable practice. I think it is not a little remarkable that in Dr. Johnston's 169 cases in which he applied the forceps before the os was dilated, he had nine deaths—that is, more than five per cent. of his cases died. But we all know that, as a rule, delay in the first stage of labour is not attended with such an amount of risk as this. Dr. Churchill collected the details of a large number of cases in which there was great delay in the first stage of labour; he considered no case as lingering unless it had exceeded sixteen hours in the first stage, and his cases ran from sixteen hours to 176 hours; and of some 1160 cases of which he collected the full notes—not selected cases, but consecutive cases—in which this occurred, there was not a single death, not a single case exhibited any evil consequence of delay in the first stage of labour. Dr. George Johnston himself has placed before us a large number of cases in which there was delay in the first stage, and in his report of the lying-in hospital under the mastership of Dr. Shepperton—a report of Dr. George Johnston and Dr. Sinclair—he gives us the details of 91 cases of delay in the first stage of labour; and I think in these cases there was not one death of the mother, nor a single evil consequence of delay. Contrast that with the result of the practice of using the forceps in the first stage, where 5·22 per cent. of deaths took place. There is another point that seems to me to illustrate very strongly the danger of this practice of using the forceps in the first stage of labour, and that is that the proportion of deaths bore a striking relation to the smallness of the os uteri when the instrument was applied. If you examine Dr. George Johnston's cases, the first thing that strikes one about them is that all the deaths were in the first labours; there were no deaths occurring in second labours—I am speaking now of the application of forceps during the first stage of labour. Altogether he used it in 169 cases; in 59 the os was dilated only two-fifths of its extent—that is to say, the os was dilated to a diameter of about an inch and a half; 44 of these cases were primiparous patients, and 15 of them were pluriparous. Of these 44, 6 died, being at the rate of rather more than 13 per cent. of the patients in which the forceps were used, the os being dilated two-fifths. If we exclude two cases of convulsions which were brought into the hospital in a comatose condition, out of 42 patients, 4 women died, being at the rate of 9·5 per cent., where the os was only dilated two-fifths. In his next class of cases, the os was dilated three-fifths. There were 71 of these cases delivered; 53 were primiparous, and 18 pluriparous. Of the 53 primiparous patients two died, or 3·77 per cent. None of the pluriparous patients died. When the os was dilated to four-fifths, excluding a case of convulsions brought into the hospital in a comatose condition, none of the patients died. One of the primiparæ was brought in in a comatose condition, and she died, but certainly not as the result of the application of the forceps. I say this affords very strong evidence of the danger attending this operation, and the danger is exactly in proportion to the smallness of the os at the time the operation is performed. I think there can be no doubt that we are greatly indebted to Dr. Johnston for having shown us that we can deliver a patient with the forceps in the first stage of labour. Although the practice may have been adopted by others, he certainly is the man who has taught the profession that it can be done, and he has shown us that we have in certain cases this alternative, that if the necessity arises we can use the forceps in the first stage, and deliver the patient with, perhaps, much greater safety than we could leave her undelivered. But to follow out the practice, and adopt it as freely and generally as he has done, would, I maintain, be attended with danger and risk to the patients. I came here, Sir, for the purpose of hearing the observations of my London brethren, and I scarcely meant to occupy your time at all. We have discussed the subject very fully in Dublin, and we have, of course, taken different views; but I hope that the discussion to-night will be one that will put the matter in clear and full light, and will serve as some guide to the profession. But to let it go forward as our teaching that we may always use, or should always use, the forceps early in the first stage of labour, when there is no urgent demand for it, except the time that has been passed, would, I think, be most unfortunate for Society, and for our profession.

Dr. THORBURN (of Manchester). — As you were kind enough to call upon those who have come from some little distance to offer any observations upon this subject, I cannot avoid rising to thank Dr. Barnes for the very able manner in which he has placed it before us. I am very glad to find that he has not endeavoured, as some have endeavoured, to put before us in anything like a statistical or definite way the frequency with which the forceps should be used in the low operation. I believe it is utterly impossible by any amount of speaking or writing to do anything of the kind, and that the only principles upon which we can regulate our practice in this respect are to be guided by two rules. The first of these rules is never to use the forceps solely, or at all I may say, for our own convenience—to save our own time; and the other rule is, when the parts are fairly dilated, never to allow an unfortunate woman to undergo a long period of tedious suffering which we can at once mitigate by an operation that is perfectly safe. Beyond this I do not think it is at all possible to go. I feel myself personally indebted to Dr. Barnes for having given the high weight of his authority to the practice of using the forceps in the high operation, when the os uteri is not fully dilated; and I do not think Dr. Kidd has exactly opposed himself to what Dr. Barnes said on the subject. No doubt there must have been a considerable amount of nonsense, I should say, talked on this subject in Dublin—I do not mean by Dr. Kidd; but if it has been suggested by anyone that it should be at all a common practice to introduce the forceps into the uterus when the os uteri was only about an inch or so in diameter, and apparently for the mere purpose of dilating the os uteri, that is not what Dr. Barnes would recommend. It is perfectly within the knowledge of all of us that there are a good many cases where, owing to the pendulous abdomen, or to some slight disproportion, or to some cause which we cannot diagnose, the head remains for a very long time obstructed at the brim, unable apparently to descend; the os uteri, although moderately dilatable, is not widely dilated, and yet it is perfectly apparent that it is not the obstacle to delivery. In these cases, if you allow too long a time to elapse, you allow the head to be pressed against the higher portion of the uterus, against a segment higher up than the os uteri, and you incur very great danger in that way. I am quite satisfied that the forceps under those circumstances can be easily and safely introduced; that if once placed fairly upon the head, if a powerful pair of forceps be used, which always means a gentle pair—if these be used steadily and slowly, according to the emergencies of the case, not merely for the purpose of dilating the os uteri, but having to do so to complete the dilatation in the progress of the descent of the head, I believe under these circumstances the os can be as slowly and as safely dilated in this way as it may be even by Dr. Barnes's hydrostatic bags themselves. This, I think, is really more what Dr. Barnes wished to impress upon us. It is a practice which I should wish to see held before members of the profession. I wish in doing so he had spoken a little more firmly and strongly against the practice of using ergot as an alternative under these conditions—that is to say, when the os uteri is not dilated. Now, we are very much assisted in such an operation as this by the use of antiseptics and chloroform, and I think some little confusion has arisen in all our discussion upon operations of this kind in midwifery by forgetting that we sometimes endeavour to compare the dicta of men who either have not, or did not, use chloroform with the dicta of men who do or did use it. When we do that, we are perfectly certain to find a number of apparent contradictions which are not real, because I am quite sure it must be the experience of everyone of us that in the most serious forceps operations forceps with chloroform is a very different affair from forceps without it. I do not mean, of course, to endorse the wonderful dictum promulgated at one time by one of our teachers, that we should not use chloroform in order that we may know by the pain given when we are forcing the edge of the forceps through the vagina or the uterus; but, on the contrary, I believe there are many cases suitable for forceps if chloroform be given which are not and could not be suitable for it if chloroform be not given, on account of the comparative undilatability of the parts. There is just one other point which has struck me as of some importance, and that I do not remember ever to have noticed among medical men, though I once mentioned it to a Society of our own at Manchester, which, I believe, throws some light upon the use of the long forceps operation—that is to say, the use of the forceps at the brim, independent of the presence of the os. The British forceps, as Dr. Barnes in his work has so forcibly pointed out, is probably quite as effectual as those used in France, or largely in America, as a tractor, and also as a lever; but undoubtedly the British forceps, as we have them in the best of them, are decidedly inferior to the French forceps as compressors. It is chiefly in the more serious forceps operations that we do want and must have the compressing power of the forceps; and I should like to point out a very simple way indeed in which we can increase almost indefinitely the compressive power of ordinary English forceps under such circumstances. If you apply the blades of the forceps to the head and grasp the handles—tie them together if necessary; it is not a good practice as a rule—you get then as much compression as the instrument seems to be capable of producing, and if you allow for the spring of the instrument, it is not a very great deal. But supposing, instead of grasping the whole handles, you only fix together the extreme ends of the handles, either by a slip or a piece of tape, then if you will hold the forceps up and stick something small between the other ends of the handles, so as to separate them as you force these two handles separate, you bring the blades of the instrument together, and in this way, by a little thickness not much greater than the thickness of this pencil introduced between the upper ends, while the lower ends are tied together, you bring the two blades of the forceps much closer together. I have produced this result in this way: drilling a hole through the one handle and fitting a screw to it. It is, of course, only used in comparatively rare cases. Meeting, as I have done, with cases of the brim, where I had almost entirely been foiled by the use of the forceps in the ordinary way, I have fastened together the lower ends of the handles, and with this screw have separated very slowly the upper ends of the handles, and in this way brought the handles together to a very much more considerable extent. By this means on at least two occasions I have succeeded in delivering two living children, where I had otherwise given up all hopes of doing so. Although, of course, I use a considerable amount of pressure, I think this is more safe both for mother and child than the other alternative of turning in such a case.

Professor STEVENSON (of Aberdeen).—I shall endeavour to show my appreciation of your kindness and the honour you have done us in calling upon us to take part in this debate early by making the remarks which I have to make as briefly as possible. It has afforded me much pleasure, certainly, to be present on this occasion and to meet with so large a number of the representatives of midwifery, not only of the metropolis, but also of the provincial towns and the sister island; and I regret that I am the only one present who may be said to represent the Scotch school of midwifery. I will, however, still claim Dr. Matthews Duncan as a representative of the Scotch school, seeing that it is not so long since you stole him from us; and he made his great reputation in Scotland. However, I think it is but right that the Scotch school should be represented, seeing that it has special characteristics which I think have important bearings upon this discussion. We have one special characteristic which I think has this special bearing—that is, we have double the time of teaching which the other schools have. It has also been the characteristic of the Scotch school from the days of Burns and Hamilton, which take us back to the beginning of this century, to promulgate the doctrine that ineffectual contraction of the uterus should not be tolerated, that it does injury to the patient, and if you cannot help on labour you should arrest it. This has been fully borne out by the researches and teachings of Sir James Simpson, who, perhaps more than any other, established the doctrine that prolongation of labour is extremely injurious to the mother. Then, again, I cannot forget the fact that this discussion really originated in Scotland; the keynote of the discussion, at all events, so far as the early use of the forceps is concerned, was really sounded there. Although the general practitioners of Scotland may, perhaps, not use the forceps so frequently as Dr. Hamilton or Dr. Barker would recommend, yet I must say, from my acquaintance with the general practice of my brethren, I know, as a rule, they do use the forceps frequently and early, that they regard them as safe, and, in fact, that in practice they have followed out the principles so ably laid down by Dr. Barnes, that in the forceps we have the best means of counteracting the dangers attendant upon the protraction of labour or undue disproportion of time in labour. It has been argued

against the position which we take up regarding the early use of the forceps—and remember, by the term "early use of the forceps" I mean the use of it whenever you are convinced that labour is becoming dangerous, not waiting to see if it will finish without danger, but whenever you are convinced of its becoming dangerous,—I say the early use of the forceps has been objected to upon the ground that it is dangerous to give our authority for this practice in case it may become injurious in the hands of the inexperienced. But I have always felt that in my teaching I could not lower the principles which I promulgated to the level of the inexperienced, but rather should bring the inexperienced up to the level of the experienced. The frequency of the use of the forceps is quite a different thing from its early use, and when I hear of the forceps being used once in every five cases, or once in every eight or in every ten cases, I feel, not that they have been improperly used under the circumstances, but that the labour had not been properly attended to; that the powers of that patient had not been conserved properly, and that the attendant had failed in his previous management of the case. It is by the previous management of the cases that we can prevent the too frequent use of the forceps; it is by thoroughly educating our men to understand, not only what is the normal mechanism of labour, but what is the effect of the management of labour; and if we are to get what Dr. Barnes has happily termed a "scientific frontier" against the gross use of the forceps, it is by the thorough education of our men—not education merely in the use of the forceps, but education throughout the whole system of surgery—the science as well as the practice of midwifery. And now, to show the bearing of my observations upon the single question of the use of the forceps in the first stage of labour for the dilatation of the os. Here, I think, we have got into thorough confusion, from the want of relying upon what we have called the "scientific frontier." It is this. The size of the os is no criterion, to my mind, of the progress which the first stage of labour has made, or that the labour itself has made; and I think that those who oppose Dr. Johnston have not fairly represented him, and that he has placed himself at a disadvantage by taking the size of the os as the criterion when he will apply the forceps. That the size of the os is not the criterion which should be taken, is manifest enough from the fact that he says he would frequently begin to use it when it is only the size of an inch or thereabout. We have still to get a good deal of knowledge before we can decide this question. There are other conditions that are taking place besides that of the dilatation of the os, and it is but recently we have become acquainted with a most important change that is going on, and that is the elongation of the cervix. We hear men in discussion continually confounding cervix and os, as if they were the same terms. The os may still remain very small, and yet the cervix may be thinned and attenuated, and the patient may be in extreme danger. What we require to do is to know what are the changes that have taken place above the os, or, as I prefer to call it, the parturient ring, for by using that term you do not confound the mere os and the cervix. I say, what you require to know and determine is the changes taking place with reference to the cervix, and not with reference to the mere os. If we find that in cases there is evidence of the cervix being thinned and attenuated, then we are bound to use the forceps, and delay in the use of the forceps may end in danger of rupture of the uterus, and that is just the position that Dr. Johnston takes up. He has not, I think, ever promulgated the doctrine that you are to use the forceps in such cases as Dr. Kidd has mentioned, where the membranes are still there. The membranes, he tells us, must be long ruptured, the liquor amnii must be away for a considerable length of time. In addition to that the head must not be coming down; he must find that the os is not dilating, because, the membranes being away, the head cannot be kept down to dilate it, and that is just the very condition in which the cervix uteri becomes thinned out and attenuated, and there is real danger. That, I consider, is the most important point with reference to this discussion to-night. I will conclude by just quoting another rule in addition to that which Dr. Thorburn gave, a rule which I give my students to guide them in the use of the forceps—that is, to treat every patient in the same way as they would either do themselves, or have another treat their own wives.

Dr. NEWMAN (of Stamford). — I have listened this evening, not only to the excellent paper which Dr. Barnes has read to us, but to the opinions of other gentlemen who have spoken from their special standpoint as teachers of obstetrics. If I have any claim to speak to you to-night it can be in no capacity of that kind, but simply as a practitioner of obstetrics, and that, of late years, to a comparatively small extent. Still the subject is one that has interested me for long, and if I seem to be somewhat egotistical in what I say I hope you will pardon me, and take it to mean that, speaking, as I must, from a narrow point of view, I am still trying to convey to you the impressions that have, by slow degrees, formed themselves in my own mind. Some twenty-five years ago my student course ended, and the obstetric teacher to whom I owe so much was the late past-president of our Society. I need hardly say his teaching throughout was, to say the least of it, not in the direction of rashness. For a number of years his dicta held weight with me, and I am almost disposed to say now that they held weight with me almost too much. I remember well the first case of operative midwifery I saw was the case of an out-patient of St. Bartholomew's Hospital. Labour had gone on for some long time. The operation of craniotomy was performed, and some six or seven days afterwards I had the wretchedness of having to report the escape of a large slough and the subsequent formation of a large vesico-vaginal fistula. The years that have passed since then have rather tended to make me feel that in the forceps—and I am purposely for one moment ignoring the difference between high and low operations—we have an instrument of enormous value, and one also at the same time that admits of very much more extended use than perhaps a school of caution would be disposed to allow. A gentleman who is not a member of this Society, but who is a very great friend of my own, and enjoys far more practical opportunities than I have done or am likely to do, speaking on this matter a few days ago, said, "I think I could send you a note of the number of times per thousand in which I have had recourse to the forceps." His note ran thus: In the first thousand cases he used the forceps ten times; in the second thousand cases he used the forceps forty-eight times; and in the third thousand cases eighty times. I do not know that I have ever met with a more careful, more shrewd—I use the term advisedly—a more wise midwifery practitioner. Then, again, I think one should to-night, at all events, take full cognisance of this. I am glad it has been so fairly laid down that no formulæ can be supposed to do more than approximate to the truths that we have before us, because it is a matter of daily experience that the same thing is looked at by two men, or by many men, in many different ways. "Quot homines, tot sententiæ." That is true enough with regard to matters of everyday life, and it is certainly true with regard to the matter of which we are now speaking; therefore, what to one man is comparative caution, would, to his neighbour differently situated, be looked upon as, at all events, a degree approaching to actual boldness or rashness. Then, again, I think one has markedly to bear this in mind. I do not know how you are to separate between the local result of an inquiry that nature will inflict in a prolonged case of labour and the result that may follow—and no doubt often does follow—where the forceps are resorted to on account of immediate and urgent danger. One man may, with full truth, put down to the use of the instrument that which, after all, has been done before his instrument was even taken in hand. It would ill become me to attempt to criticise any statements that have been put before you; only let me again, reverting to my friend's statement, say that I think, as years grow upon us, personal caution certainly tends to increase, and that when one finds that men actively engaged in their profession in country districts make use of the forceps, conscientiously believing that they are doing the best thing for the poor women that come under their care, the concensus of opinion gathered from men of that sort must be allowed to have its weight. Remember that we are living in the country—do not think I am going to indulge in anything like a eulogium of countrymen as compared with metropolitans for an instant, but we, at all events, are put in very different positions; we have to develop a degree of definite individuality that hardly belongs to a man who has some very much more experienced men than himself within easy call. Therefore, I simply say in a matter of this kind, I think the ill-defined conclusions at which many of us at work in the country certainly have arrived in the question before us—conclusions which agree thoroughly with the paper put before us this evening, and conclusions which I fancy each of us have for ourselves

worked out—are, at all events, deserving of recognition and consideration as fitting evidence of the value of the instrument of which we have to speak, applied even more frequently and with less hesitation than the more cautious teaching of a few years past would lead one to adopt.

Dr. MALINS (of Birmingham).—As the area outside the metropolis appears to be in the ascendant this evening, perhaps you will allow me to add my testimony, in the first place, to the pleasure already expressed, with which we have listened to the paper read by Dr. Barnes, and to the simple and clear manner in which he has enunciated the subject for discussion. He remarked that the practice not only of individuals will vary, but also of localities; and thus, while the teaching of one school has been the preponderance of the operation of craniotomy, with all its dreadful elements, another school has advocated the use of the long forceps; and experience has shown that the latter has given the best result both to parents and offspring. It is a singular fact that during a pupilage of six years in a very populous district, with from 400 to 500 midwifery cases in the year, I never saw a case of craniotomy. During a residence of four years in Edinburgh, part of which time I was intern at the lying-in hospital, I never saw a case of craniotomy; and in my own practice of nine or ten years I have never witnessed a case of craniotomy; showing that the necessity for craniotomy does not exist in some parishes or in the minds of some individuals as it does in other places. Reverting to the frequency of the use of forceps in practice, probably no greater impetus was given to the discussions on this subject than by the paper written by Mr. Harper in the first volume of this Society's Transactions for 1860. In 1870 I read a paper before the Birmingham Branch recording 806 cases where the forceps were used once in twenty-two cases; and during the past five years I find that I used the forceps once in every five cases, being influenced partly by the considerations of my practice and partly by the teaching of others. In that calculation I have omitted cases that occurred in my hospital practice and in the practice of those with whom I have seen patients; and in my own practice pure and simple, during the last five years, I find I used the forceps once in five times with the best results. Dr. Kidd has stated that it was the teaching of his school that prolongation of the first stage of labour gave rise to no bad results. The opposite doctrine to this was very clearly demonstrated by Hamilton and Burns, of Edinburgh. Hamilton was instructed to say—and in that he was followed by Sir James Simpson—that the most disastrous results followed. You not only got wearying of the uterus and irregular contraction, but you had a greater proneness to hæmorrhage. This is a teaching which the experience of most men in active practice will fully bear out. With regard to the subject of the use of alternatives to the long forceps, with the head at or above the brim, he has shown that there are certain indications which perhaps point to the necessity of the use of the forceps; but there are other indications which point to the expediency of other operations. There are conditions of sudden catastrophes on the part of the mother in which delivery may be effected more safely and expeditiously than by the forceps—that is to say, by turning. I will not go so far as to advocate the practice of turning in natural labour, as advocated by some, and as practised by Dr. Begg, who turned most of the natural labours with which he came in contact, and with the best possible results; but what I do maintain is that in some cases turning may be very much more readily effected than the use of the long forceps, where the head is above the brim. In certain cases of disproportion, from the large size of the head, and slight contraction of the brim, or from the condition where the arm is placed behind the head in which no amount of direct force will overcome that difficulty, then it will be found that turning is a wiser and safer plan than the application of the long forceps. If there is the amount of truth which I assume there is in the statistics which Dr. Kidd has quoted to-night, that the mortality in high cases in primiparæ is so very large and dangerous—if the average mortality is assumed to be one in thirteen of the mothers, and one in three of the children,—then as an alternative to the use of the forceps for the high operation, the practice of turning affords a safer and better prospect to both mother and child. There are also conditions where it is not possible to have forceps in readiness, where it is not possible to have anæsthetics and other conditions at your command which should accompany the use of the forceps; therefore in the majority of cases in which the high operation is used I maintain that the alternative of turning is as efficient, as easy, and as safe a practice as the use of the forceps.

Dr. ALDERSON.—It is with great diffidence I rise to address the Society, but I feel I have something to say, and that in justice to Dr. Barnes I ought to say it, for since I have read his book my practice has been very different and my life much happier. I have taken out the number of deaths I had when I scarcely used forceps at all and when I used them rather frequently, and I was surprised at the number of still-births I had when I did not use the forceps. With regard to deaths, I had none in the cases where I used the forceps, and the recovery of my patients has been very much greater. I would also like to observe, as regards the length of the labour, there is no doubt at all that the danger is according to the length in a very great number of cases, and I am quite sure when I was a student I erred from fear of the forceps. I was as much afraid to take the forceps in hand as I should have been to amputate a leg or thigh. In 1865 I attended twenty-four cases, used the forceps once, and had two still-births; in 1866 I attended thirty-six labours, did not use the forceps at all, and had two still-births; in 1867 I attended fifty-six labours, used the forceps twice, and had seven still-births; in 1868 I had fifty-one cases, did not use forceps at all, but had three still-births; in 1869, out of ninety-two cases I used the forceps only once, and had five still-births; in 1870, out of ninety-six cases I had twelve still-births, no forceps; in 1871 I had eighty-two cases, used forceps four times, and had eight still-births. Therefore, during that period, in 437 cases I used the forceps eight times, with thirty-nine still-births. I may say I used ergot very frequently. In 1872 I attended ninety-four cases, used the forceps ten times, and had twelve still-births; in 1873 I had eighty-nine cases, used the forceps five times, with two still-births; in 1874, out of 114 cases I used the forceps twenty-two times, and had three still-births; in 1875, out of ninety-six cases I used the forceps fifteen times, and had four still-births; in 1876, out of 100 cases I used the forceps eighteen times, and had only two still-births; in 1877, out of seventy-eight cases I used the forceps seventeen times, with only two still-births; in 1878, in seventy-five cases I used the forceps twelve times, and had only one still-birth. Therefore, in these seven years, in 636 cases the forceps was used ninety-nine times, with twenty-six still-births. That will show you that the number of still-births is very few comparatively, if I were to exclude 1872, when I unfortunately had two or three puerperal cases. I am quite sure that there have been very many lives saved by the application of the forceps. I wont say I did not use them occasionally when they were not absolutely required, but I used them with perfect confidence, and I used them perhaps to acquire skill. I was quite certain I was doing no harm.

Mr. WORSHIP (of Sevenoaks). — I remember taking for two months Mr. Bailey's practice at Thetford, and I am very much surprised to hear, in the account Dr. Barnes gave, what a very few times he seems to have used forceps. I remember, when I went to the first case, I was only detained two hours; but the first question he put to me in the morning was, "How was it you were so long at that case last night?" I said I was not long. He said, "Had you the forceps with you?" I said, "No"; and he told me ever afterwards to take the forceps, and certainly in the two months I was with him I applied more forceps than I have ever done in any ten years since, for every woman there expected to be delivered with the forceps. Every woman used to say, almost as soon as she was in labour, "If dear old Mr. Bailey were here, he would soon put me out of my trouble." The consequence was, I should think, in every other labour I attended I used forceps. It was his universal practice. I do not think he ever had a dose of ergot with him, or ever used it, but it was one continuous use of the forceps. His expression to me was, "Well, if the child is up a little higher, you have nothing to do but to push the forceps up a little higher; if it is low, you can very easily deliver with these forceps." Of late years I have never used them, although he gave me a pair. When I have required forceps I have used Denman's short forceps, but of late years I must say I have used the forceps very little. When assistance has been required, I have used the whalebone loop, and have had comparatively few cases of flooding. I believe it is simply the tired uterus which floods, and not the precipitate labours, as we used to be taught in the books as being the cause of hæmorrhage.

On the motion of Dr. Edis, the discussion was adjourned till the next meeting on the 4th of June.

THE LANCET.

LONDON: SATURDAY, MAY 17, 1879.

THE paper read by Mr. WATSON CHEYNE at the last meeting of the Pathological Society is a valuable contribution to the lengthening list of observations upon septic processes and antiseptic measures. It was especially noteworthy for clearing away in a most thorough manner certain ideas that have sprung up in regard to each of these subjects. The title of the paper was the Occurrence of Organisms under Antiseptic Dressings. This was sufficiently startling, for in its bald simplicity it seemed as if the first principles of antiseptic surgery were about to be abandoned, when one of Mr. LISTER'S own pupils, carrying on investigations upon cases treated by the founder of antiseptic surgery himself, had declared that organisms could and did occur in discharges from wounds kept antiseptic. But, as it turned out, the elaborate inquiry, instead of overturning antiseptic surgery, only served the more to strengthen it, whilst it brought to light some very remarkable and interesting facts concerning the lowly organisms with which it dealt. Stimulated to investigation of the subject by the attacks against the antiseptic theory in Germany, based on the alleged existence of organisms in the discharge from wounds maintained under antiseptic dressings, and whilst believing that these observations were open to criticism, Mr. CHEYNE commenced to examine rigidly the discharges in all cases treated by Mr. LISTER. The discharge from a wound kept thoroughly antiseptic is almost clear, odourless, and contains some granular matter and degenerate leucocytes. It being impossible to distinguish any minute organisms in such a fluid by the microscope alone, he had recourse to the method of "cultivation," and inoculated flasks containing infusion of cucumber with such discharges under strict precautions. The result was, that whenever the discharge was taken from a case in which the dressings had not been changed for a few days the inoculated fluid became turbid in from thirty to fifty hours, and on examination was found full of the minute spherical organisms known as micrococci. If, however, instead of the "sweet" discharge from a perfectly antiseptic wound, some of the foul product from a wound which had not been kept antiseptic, and had consequently undergone putrefactive changes, was employed for the inoculation of the cultivation-fluid, there were developed, not micrococci, but rod-shaped bacteria, and this so constantly as to point to some general relation between the bacteria and the putrefactive changes not held by the micrococci. It pointed also to the probability that these two forms, the spherical and the rod-shaped, are integrally distinct from one another. Experiment bore out this view, for it was found by Dr. GERALD YEO that, whereas the fluids containing bacteria killed rabbits on the fourth day after their injection into the jugular vein, the injection of the micrococci-containing fluids was harmless. Thus was explained the surprising fact that organisms, if micrococci, could exist under antiseptic dressings, and give rise to no local or constitutional disturbance; whereas, if bacteria found an entrance, they would lead to putrefactive changes and all the grave sequelæ of local inflammation and general constitutional symptoms, which set in often in cases where antiseptics fail, or where they have not been applied.

This discovery—for so indeed it is—was, however, only the starting-point for further research. It had to be shown, if possible, what was the reason for the flourishing development of micrococci under antiseptic dressings to the exclusion of bacteria; and the even wider question, as to how any organisms at all could find their way to wounds so thoroughly protected as Mr. LISTER'S cases are. Dealing with the larger question first, Mr. CHEYNE pointed out that the organisms might gain access to the wound by means of the circulating blood, or by means of some imperfection in the antiseptic method, or by spontaneous generation. The last of these three alternatives was dismissed in a few words by Mr. CHEYNE pointing to the fact that he had kept infusions and organic fluids for months until they dried up, under varying conditions of atmosphere and temperature, and without any organisms arising in them spontaneously; and he pertinently asked why, in the same subject, under the same conditions, organisms appeared in the discharge from one wound at a different time from that they appeared in another; and said that if their appearance depended on external conditions all wounds in the same ward should show them at the same time. The question as to their introduction through the medium of the blood was not so simply disposed of. In the first place, it must be remembered that although it is generally believed that healthy living blood is incapable of supporting the growth of organisms, yet the short time in which, after death, such organisms have been found in the midst of healthy organs and tissues has led to the belief that they must have gained entrance during life. And great stress has been laid upon experiments of imbedding organs rapidly removed by red-hot knives from a newly-killed animal in melted paraffin, and finding in the interior of structures so protected a large number of bacteria. Mr. CHEYNE, repeating these experiments, but on a different plan and with more precise precautions, did not once succeed in finding organisms in the structures in question, unless the animals themselves had before death been inoculated with fluids containing micrococci or bacteria; in which case micrococci or bacteria were developed in the interior of organs. But if healthy blood is incapable of affording pabulum to these organisms, it is different with blood in diseased states. Acute inflammatory processes, or chronic nutritive disorders, equally lay the body open to favour the development of organisms—generally micrococci, sometimes (but rarely) bacteria. So he concluded that only in weakened and diseased conditions could the organisms he found in the discharge from protected wounds have gained entrance from the blood.

This narrowed the question to one of efficiency of the antiseptic method, and here Mr. CHEYNE has again added much that is new and of great importance. He proved fully that both the lotion and the spray of carbolic acid are perfectly destructive of the organisms, whether bacteria or micrococci. But it would seem that the dressing gradually loses its antiseptic properties, partly from evaporation of the carbolic acid, partly from combination with the albuminous

material of the discharges. This, of course, has always been recognised; the importance of frequent renewal of the dressing in proportion to the amount of discharge, and the liability for this latter to become offensive if it be allowed to soak completely through the dressing, being well-known matters of practical experience. What Mr. CHEYNE showed was that micrococci occurred in the discharge long before this soaks through the dressing, and this he found to be due to the fact that these minute organisms can flourish in fluids with a certain admixture of carbolic acid—viz., 1 in 500. More than this. By a series of experiments, using cultivation fluids mixed with carbolic solutions in varying degrees of dilution, he found that by a gradual process of successive inoculations he could get micrococci to flourish in dilutions of 1 in 300 instead of 1 in 500, as if by growing in the presence of the acid the organisms increased in vigour, so as to be able to withstand the influence of far less diluted solutions of the acid ultimately than at first.

Still, there remained the question—Why, if organisms such as micrococci can thus penetrate through the protection afforded by carbolised dressings, do not the less innocuous bacteria also penetrate? To this experiment again gave answer. He found that whereas bacteria flourished more rapidly than micrococci in the pure infusions, in the infusions to which carbolic acid (in a proportion of 1 to 500) was added, the micrococci grew the more rapidly. He also found that the one which was in the most favourable medium to itself developed to the exclusion of the other, if a mixture containing both forms were used for the inoculation. In other words, in the presence of carbolic acid, micrococci flourished, at the expense, and to the exclusion, of bacteria; whilst in unprotected fluids the reverse took place.

The manner in which the research was conducted, and in which each question as it arose was combated, is worthy of all praise. Although not quite disposed to admit that Mr. CHEYNE has fully succeeded in showing a complete specific difference between micrococci and bacteria (he admits that *all* forms of micrococci may not be harmless, and quotes the recent researches of KOCH upon the pyæmia of rabbits as proving this), he has certainly established in a very satisfactory manner that organisms can and do occur under antiseptic dressings; that so long as these organisms are what we call micrococci, no ill results accrue from their presence, but that if they be bacteria, then the evils of putrefaction are set up. The test of thorough antisepticism rests, then, no longer upon the absence of organisms from discharges, but upon that of a special form—the bacterium.

THE Government has earned the thanks of the profession by another great concession on the subject of Medical Reform. In answer to Mr. ERRINGTON, whose question we reported last week, the Government has consented to refer the whole question of Medical Reform to a Select Committee. Our readers will remember that prior to this, and on the occasion of the motion for the second reading of Dr. LUSH'S Bill, the Government gave an undertaking that the question of the constitution of the Medical Council should be referred to a Select Committee, and that the Government Bill should not be proceeded with till that Committee had given in its report. This concession did not satisfy those who thought that the constitution of the Medical Examining Boards of the future was a subject as difficult, as important, and as worthy of investigation by a Select Committee as the constitution of the Council. These are, in fact, the two halves of a great subject, and the reference to a Select Committee of one-half without the other would have been a narrow and incomplete reference. We congratulate Mr. ERRINGTON on the success of his question, and we congratulate the Government on having had the wisdom and the magnanimity to yield to the pressure put on it from many different quarters. It is to be gathered from the language of Lord GEORGE HAMILTON that the Government has yielded rather reluctantly, and somewhat against its own judgment. It was not to be expected that it would immediately see that the question was more complicated and difficult than at first sight it appeared to them, whether from the view of the corporations and universities concerned, or from that of the public. It was natural to think that so obvious a scandal as nineteen bodies for licensing to practise medicine, with various and conflicting interests, could be easily removed. But by this time the Government itself must see with what tenacity the nineteen bodies adhere to all their privileges; how they refuse to be reduced to three; and how, even if that reduction must come, they insist on such pecuniary terms as will perpetuate each of them in undiminished wealth, and on a permanent individual representation in the General Medical Council after all their individual importance shall have been merged. At any rate all this must come out before the Select Committee if the investigation by it is to be thorough, and if it is not to be thorough it had far better not be at all. We think all the more highly of the Government for its reasonableness throughout. Every successive concession has been an improvement—not of course in the judgment of bodies who look at change through the coloured spectacles of their own interest—but in our judgment, looking at the subject from a broad professional standpoint, and from that of the public. The Bill at first was merely a permissive affair. The introduction of compulsion gave it the look of law and earnestness. The reintroduction of the principle—embodied in the first draft of the Lord President's Bill—that the responsible authority in the matter of curricula, examination rules, &c., for the future, must be the General Medical Council was a great step in advance, though taken in direct opposition to the wish of the Medical Council itself, which shirks responsibility. The undertaking of the Lord President to endeavour to secure a uniform system of fees, as well as to improve the nature and severity of the examinations under the Bill, was another indication of earnestness on the part of the Government. In fact, there is little doubt that if the Government had not been hampered so much with the advice of the Medical Council, based on consideration for the nineteen bodies, it would have been able to devise a Medical Council and an examining system that would have satisfied both the public and the profession.

Even now, with the prospect of a Select Committee, the investigation may be so conducted as to yield very unsatisfactory conclusions. If the witnesses examined are only such as are members of the Medical Council, or members of the Council of one or other of the Corporations concerned, it will be easy to find excuses for continuing existing arrange-

ments or minifying the changes to be made. But we will hope for better things. The Government has given by its concession on Monday night, even if reluctantly, a fresh proof that it is open to reason and representation, and is desirous of carrying this question to a legislative consummation. The present scandal is a great one. It will be mitigated only in the degree in which the Medical Council and the medical examining bodies are made independent, and all money considerations are eliminated from the question. If the Committee to be nominated is such as to guarantee such results, we venture to say that the Government will lose no credit and no gratitude in connexion with a subject of real importance to the public and to medical science.

OUR readers will be surprised to learn that the British plague-commissioners, Surgeon-Major COLVILL and Dr. PAYNE, are reported to be on their way homewards; and they will, perhaps, be still more surprised to learn that their backward route is by way of Central Russia, and not, as was anticipated, by way of the Caucasus, Poti, and Odessa. Both gentlemen, we understand with regret, have suffered more or less from malarious fever in Astrakhan, and this may have had something to do with their premature return. But the information that has reached us leads to the conclusion that they have been summoned homeward by the Privy Council, in consultation with the Royal College of Physicians, under the belief that their work was accomplished. Indeed, if a paragraph which, in some mysterious manner, has found its way into the *Leeds Mercury* be accurate, the two commissioners have already reported to the Privy Council on the only point left open to them for independent work when they arrived on the Volga—namely, the origin of the plague at Vetlianka; and that in the sense of the German and Austro-Hungarian Medical Commissioners. These latter gentlemen, although admitting that the question is still a moot one, have come to the conclusion that the plague had probably been brought to the banks of the Volga by Cossack troops returning from the war in Asia Minor; and the British Medical Commissioners would appear to have accepted this conclusion.

Now both this conclusion and the more defined and important data bearing on the subject given in the papers recently laid before Parliament by the Local Government Board, on the modern history and recent progress of Levantine plague, show that, in the absence of plague to detain the Commissioners on the Volga, their true field of work was in the Caucasus, and especially in certain parts thereof. That this field should not have been investigated by the British Commissioners, as would now appear, is a deplorable illustration of the manner in which this business has been muddled by the Privy Council and the Royal College of Physicians. To the latter the omission is particularly discreditable, for it is a most striking proof that, while not hesitating to take upon itself the function of advising the Privy Council, it was wholly uninformed of the abundant recent information which existed on the progress of the plague, and of the bearing of this information on the Vetlianka outbreak. But this is not all. Plague was known to have almost entirely, if not wholly, ceased on the Volga before the Commissioners left England, and it was presumed that their instructions would have special regard to this fact; so that after they had got together such information as was to be ascertained on the spot concerning the recent outbreak, they would be directed to investigate exhaustively the question of origin, while waiting to see whether, with the advancing spring, any reappearance of the disease would occur. Russian physicians apprehended such reappearance in the course of May or June, and this reappearance was dreaded, not merely on the Volga, but also in parts of the Caucasus where anomalous bubonic disease had been observed. As we write, the news comes from Tiflis of a very fatal unnamed disease having broken out among the Caucasian range of mountains, and of a medical commission having been despatched into Ghilan, northern Persia, on a question of plague. But so incredibly short-sighted and incompetent has the Royal College of Physicians proved itself to be in the advice it has tendered to the Privy Council, that the British Medical Commissioners appear to have been recalled at the very time when it was most desirable that they should have been in the East, and we may expect to see them in London in a few days.

That the Commissioners will come back empty-handed we do not for an instant believe; but to recall them at the moment when their presence on the Volga or in the Caucasus gave the greatest promise of a rich return for their distant journey, and, plague having ceased in Russia, without their having visited the localities it was most important that they should visit, is simply to stultify their appointment. This stultification is not the less aggravating because it is a normal ending of a transaction discreditable from the beginning alike to the Royal College of Physicians and the Privy Council.

THE present session of the Royal Medical and Chirurgical Society will be chiefly remembered in future years for the discussion that has taken place on the identity of membranous croup and diphtheria. Partly as a result of Dr. SEMPLE'S paper of last year, and partly because of lectures in our columns by Dr. GEORGE JOHNSON and Sir WILLIAM JENNER some three or four years ago, a committee was appointed to gather evidence, and draw up their conclusions on this subject. The report was presented in the early part of the session, and the Society has done wisely in devoting its past three meetings to its discussion; for although neither the report nor the discussion has led to any very definite conclusion, as to the identity or non-identity of the two affections, yet they have their value in placing more clearly before the profession the present position of the question. As this becomes appreciated we may hope that any new facts which may be observed will be subjected to rigid scrutiny, and so tend to the disentanglement of the problem, instead of encumbering it with deficient and untrustworthy observations, as is now too frequently the case. We must congratulate the Committee on the excellence of their report; for it gives a clear and lucid analysis of the subject, and will remain as a standard work of reference for all future observers and writers on membranous laryngitis, even although its conclusions may appear somewhat lame and halting. To appreciate its value, and the line of discussion adopted by the chief speakers, it must be remembered that the scope of the inquiry

was strictly limited to the determination of the question, whether such a disease exists as "idiopathic membranous croup?"—i.e., whether membranous laryngitis exists independently of the diphtheritic poison, and whether, if so, there are any criteria by which it can be distinguished clinically or pathologically. The evidence collected by the Committee consists practically of three kinds—viz., replies from about ninety medical men to a series of questions relating to the etiology and general history of membranous laryngitis; tables of cases observed by Drs. GEE, FAGGE, and DICKINSON; and the reports of inquiries into epidemic and endemic diphtheria made under the direction of the Medical Officer of the Local Government Board. A complete digest of the evidence is appended to the report, and is even more valuable than the report itself. The conclusion at which the Committee arrived is, "that there is evidence that membranous exudation in the air-passages can be produced by mechanical and chemical irritants apart from the contagious diphtheria, and that a few cases exist in which there is some evidence that membranous laryngitis has followed exposure to cold. It will, however, be seen that in a large number of cases some other factor must be invoked as a cause apart from or in addition to this—either a morbid systemic condition due to the presence of some constitutional or febrile disorder or insanitary surroundings, or some other cause not apparent. But there is strong evidence that all the cases thus produced do not fall under the head of contagious diphtheria, and that there are classes of cases distinct from that disease." The meaning of this passage has been much canvassed, and we must say that we could have wished it more clearly and explicitly worded. It would seem to have been arrived at as a compromise between those members of the Committee who had come to a conclusion that membranous exudation into the larynx was always due to diphtheria, and those who believed that it may and does occur occasionally in the course of a simple laryngitis. As is the fate of most compromises, it satisfies neither party, and is invoked by both as supporting their views. It is, however, clear enough that the Committee have committed themselves to the more generally accepted belief that there are classes of cases of membranous laryngitis distinct from diphtheria, and so have answered in the affirmative that part of the inquiry. We now come to the second part of the Committee's investigation—Are there any criteria by which it can be distinguished clinically or pathologically from the membranous laryngitis of diphtheritic origin? In determining this question we are struck, as noticed by Dr. POORE in the discussion, with the smallness of the numbers of cases in which the membrane is confined to the air-passages, as verified by post-mortem examination, and in many of these some previous illness, such as scarlet fever or measles, had been present. Primary membranous laryngitis, without implication of the fauces and pharynx, is, therefore, a rare disease, whilst membranous laryngitis associated with a similar condition of the fauces is very common. Moreover, non-membranous laryngitis is of every-day occurrence. The Committee seem to have found the difficulty of replying to this second question so great that they have practically left it unanswered. They merely state that the facts before them indicate a division between membranous and non-membranous laryngitis, but do not even tell us to what special facts they are referring. As to membranous laryngitis, we are only told that in the larger number of cases the cause is obscure. No distinctive signs are mentioned as separating the non-diphtheritic from diphtheritic cases, and pharyngeal and laryngeal membranous exudations are brought still more closely together by the statement that, except in what depends upon the position of the membrane, it is not practicable to distinguish between them, either by the construction of the membrane or the constitutional state of the patient. We must, therefore, take it as the conclusion of the Committee that, although they believe in the existence of a membranous laryngitis independent of a diphtheritic poison, they have no evidence of any symptoms or post-mortem appearances by which it can be distinguished from that due to diphtheria. We have thought it necessary to lay clearly before our readers the chief conclusions of the Committee before passing on to a criticism of the evidence and the discussion.

Annotations.

"Ne quid nimis."

SURGEON-MAJOR REYNOLDS AND THE VICTORIA CROSS.

IN the House of Commons, on Monday, Mr. Errington asked the Secretary of State for War whether, under the new regulations, the recent promotion of Surgeon-Major Reynolds, of the 24th, for his distinguished services at Rorke's Drift, would not, in the ordinary course of events, have taken place in a few months, and, under these circumstances, whether some further recognition might not be fairly made of that officer's gallant conduct, and of the prominent part he took in the defence on that memorable occasion. In reply, Colonel Loyd-Lindsay, evading the first part of the question, if his answer is correctly reported, said that Surgeon-Major Reynolds had undoubtedly done good service in South Africa, and he had been specially promoted, passing over the heads of sixty-four officers. With the promotion thus conferred, we should have been content, even although it had only anticipated his promotion in the regular course of service by a few months, had it been at all commensurate with the rewards bestowed on the two military officers, Lieutenants Chard and Bromhead. These gentlemen were promoted, and most deservedly, to be Captains, and had moreover the brevet rank of Major given them, thus raising them at once from subalterns to be field officers. We do not ask Colonel Loyd-Lindsay how many officers' heads they passed over, nor does it matter in the slightest degree—they had fairly earned their reward. In addition to this, however, they had the Victoria Cross conferred upon them. We do not think the service they performed was such as was originally intended to be rewarded by this decoration, but as the military authorities entertained a different opinion, and recommended them for it, we may fairly ask on what principle was it not bestowed on Surgeon-Major Reynolds, who had gone through all the perils and dangers of Rorke's Drift with them, who is admitted to have done good service on that occasion, who, without the slightest disparagement to the others, may be fairly regarded as having shown at least as much personal courage, and who divided his labours between tending the wounded and repelling the enemy. If the Cross were to be awarded at all, surely Surgeon-Major Reynolds ought in fairness to have been included in the list of decorated. With whom does the responsibility for this omission rest? It is to be regretted that Colonel Loyd-Lindsay did not give any information on this subject. How

did it happen that the Victoria Cross was conferred on the two regimental officers only, while all the departmental officers were overlooked? If the recommendation of Lieutenants Chard and Bromhead for the decoration was made, as we presume it must have been, by the General Commanding, it says very little either for the judgment or the good feeling of Lord Chelmsford that he omitted the names of officers who are acknowledged to have done their work unexceptionably. But we should like also to know what steps the authorities at home have taken to remedy this flagrant neglect. Has the Director-General protested against the treatment of his gallant subordinates? Has the Field Marshal Commanding-in-Chief done anything to remove the slur thus cast upon the departmental officers? Has Colonel Stanley made any move with a view to have them placed on a footing with the regimental officers? If nothing has been done, the sooner Colonel Stanley acts in the matter the better. We believe that he is really anxious to put the Medical Department on an efficient and satisfactory footing; but if he does not show that he is prepared to put an end to the false distinction of "non-combatant" as applied to medical officers, he may give up as utterly hopeless the task of endeavouring to obtain a supply of good men for the service.

THE SELECT COMMITTEE AND THE PROFESSION.

NOW is the opportunity for a great medical reform. It will rest very much with the profession throughout the country whether we are to have a really satisfactory minimum standard of examination by one board in each division of the kingdom, equal in cost and equal in severity, and whether the old taunt of having inferior qualifications, or only single qualifications, is to cease. If there is not singleness of purpose and great determination on the part of reformers the obstructives will still have the victory in the Select Committee. If nineteen portals cannot be upheld, they will still maintain that a student should pass at least two portals before he can become a practitioner, though the one be scarcely distinguishable from the other. Even the Scotch universities are so misguided as to say that all that is wanted is to compel men to get a qualification from two different kinds of medical corporations. We urge all members of the profession who wish to see the abatement of local distinctions, and of all the unseemly feelings they generate, to make their influence felt at this crisis. The profession is greater than its corporations. Henceforth let us have but one door into the profession; if we may not have but one door, let us have but one in each division of the kingdom, kept by a central responsible body, in which the wisdom of all the divisions shall be represented.

DR. CARPENTER'S FAREWELL.

AT the meeting of Convocation of the University of London, held on the 13th inst., Dr. Carpenter, in presenting the minutes of the last meeting of the Senate, took occasion to say a few words expressive of his thanks to Convocation for the kindness which he had always experienced at their hands. He said that his connexion with the University dated back for thirty-two years, nine years as examiner, and twenty-three years as registrar. During the whole of that period the interests of the University had occupied the first place in his thoughts; and it was with somewhat of a mental wrench that he found himself compelled to break off a connexion so long maintained. But he felt impelled to do this, as he wished to carry out what had long been his most cherished desire—certain scientific researches that he had long had in view. He could only do this by devoting his whole attention to these subjects, and he had thought that, both for his own advantage and he trusted also for the public good, it was better for him to lay down his office of Registrar to the University, and leave its work in younger hands. Dr. Carpenter's remarks were listened to with great attention, and received with much applause; and the first business of the meeting was the suspension of the standing orders, to allow of the following motion being put:—"That a vote of thanks be accorded to Dr. Carpenter for his long and valuable services in the post which he has so efficiently filled during his tenure of office as Registrar to the University." This was moved by Dr. Pye-Smith, who remarked that, whatever difference of opinion might prevail concerning the recent action of the Senate, there could be no doubt as to the manner in which the late Registrar had always promoted the interests of the University. Mr. Shaen seconded the motion, which was unanimously carried. Convocation then proceeded to discuss the question of the institution of examinations in the Theory and Practice of Education. In a small house, and by a majority of one, it was resolved to recommend that degrees should be granted in this subject rather than special certificates as recommended by the Annual Committee. The other business included the election of the Annual Committee, and the re-election of Dr. Storrar as Chairman.

We have before intimated our sense of the loss which the University will sustain by the retirement of Dr. Carpenter. It is enough now to express our cordial hope that he will find in his retirement the means he has wished for in carrying out those researches which may add further to his honour and be of lasting benefit to the community at large.

VACCINATION IN SCOTLAND.

THE Registrar-General for Scotland in his recently published report for the year 1878, gives an account of the state of vaccination in Scotland during 1877. From this it would appear there were registered during that year, in Scotland, 126,900 births. Of these children, 112,299, or 81,438 per cent., were successfully vaccinated in conformity with the requirements of the Vaccination Act; and 480, or 0·378 per cent., were by the regular vaccinators stated to be "insusceptible," the vaccination having been previously performed successfully, though not by persons legally qualified to grant a valid vaccination certificate. In 1304 instances, or 1·028 per cent. of all born, postponement of vaccination took place on production of medical authority. In 180 cases, or 0·141 of all born, children were pronounced insusceptible in consequence of some supposed or undefined constitutional peculiarity, and other 4 from having had small-pox. Finally, 2329, or 1·835 of all born, were lost sight of by the registrars in consequence of their having been removed by parents or guardians from the districts in which they were born. "In regard to the majority of these children," the statistical officer of the department, Dr. William Robertson, remarks, "it is to be feared that at the present time they still remain unvaccinated." A further analysis is made with respect to such children born in 1877 as were alive after the completion of their six months of life. From this it would appear that, out of 116,526 children born in 1877, and who survived for six months, 112,229, or 96·312 per cent., were duly vaccinated in strict conformity with the provisions of the Vaccination Act. If to these there be added 480 children on whom the operation of vaccination had been successfully performed by irregular practitioners, 112,709 of the children that attained the age of six months, or 96·723 per cent., were known to have been duly vaccinated. In 1304 instances, or in no less than in 1·119 per cent. of all children attaining the age of six months, vaccination had been postponed on medical authority. On these postponements Dr. Robertson observes: "It is not doubted that good reasons may have

existed for them; still there can be no question that the number is far larger than is desirable or than it would certainly be if all parents and guardians realised the serious consequences that may result from unnecessarily 'putting off' the performance of their statutory duty in respect to vaccination." The Registrar-General says of these figures that they give the "most satisfactory assurance" as to the working of the Scottish Vaccination Act, and he adds, "indeed the circumstance that, during the year 1878, there were registered in the eight principal towns of Scotland only three deaths caused by small-pox, affords strong presumptive evidence as to the efficiency of the Vaccination laws of the country."

AFGHANISTAN: THE QUETTAH ARMY.

OUR correspondent writes:—In Kandahar, at the beginning of April, the troops were settling down into summer quarters. Barracks had been found for all but a few of the garrison; and the remainder, in tents, were expected to be sheltered in houses of some description before the very hot weather set in. The troops were, with a few exceptions, remarkably healthy, due in great measure to the unusually mild winter and the almost entire absence of rain and snow. The Indian Government having decided to reduce the Kandahar force, a column under the command of Major-General Biddulph, C.B., is returning to India by a road hitherto unexplored, the Thal-Chotiali route, avoiding the Bolan Pass. The distance to Dera Ghazi Khan is about three hundred miles; the marches are long; food supplies *en route* doubtful; but the column will travel with as few impedimenta as possible, and hope to reach our own frontier by the beginning of June. The sick carriage, owing to the impossibility of obtaining fresh transport, has been reduced to 7 per cent. of strength—3 per cent. less than that authorised by the *précis* of "Field Medical Arrangements." Prior to the start from Kandahar, the sick and weakly men of the 70th Regiment, 1st Punjab Infantry, 32nd Pioneers, and 5th Company of Bombay Sappers were weeded out, so that the brigade would set out on its long and arduous march with men of good physique. Up to the 3rd of April the health of the troops in the Jellalabad Division was reported good, only 3·6 per cent. being in hospital at that date.

REPORTS OF MEDICAL OFFICERS OF HEALTH.

DR. FRANKLIN PARSONS sends us his annual report for 1878 of the combined district for which he is medical officer of health—viz., Goole urban and rural, and Selby urban. The reports are so good as to put the reviewer at fault. In other words, they are such thorough specimens of what efficient local reporting should be that it is difficult for the outsider to dissociate any fragment from its local relations. The reports generally indicate a degree of tact, temper, and power in dealing with local authorities which, in the end, must exercise the most beneficial influence over a difficult district.

The first sanitary report (1878) of the medical officer of health for the Cannock Union rural sanitary authority gives excellent promise of work, where the services of a good medical officer appear to be much needed.

Mr. John Spear's report on the health of South Shields for the year 1878 is, as customary with that gentleman's reports, of exceptional excellence. South Shields has so great an arrear of sanitary neglect to make good that it needs a medical officer of health of special parts to advise the local authority, and it has obviously got one in the present incumbent of that office.

Few reports of greater interest relating to the year 1878 have been received by us than those by Dr. William Armistead, on the several districts of which he is the medical officer of health—the districts of Saffron Walden, urban and rural; of Newmarket, Linton, Dunmow, Risbridge, and Haverhill. They abound in graphic illustrations of the conditions for the development of diseases in small country towns and in rural districts. Examples of the mode and spread of infectious diseases, and of states of water-supply, may be especially referred to. In the report on Saffron Walden (urban) an example is given of the confusion which chemical analysis is apt at times to introduce into a question of water-supply, which should have the consideration of all interested in the subject; but Dr. Armistead was injudicious in quoting without qualification Mr. Robert Rawlinson's opinion, given before the Rivers' Pollution Commission—viz., "That he had ceased to care very much what the chemists told him, or the medical officers of health, because if only a tenth part of what is said about water happened to be true, his only surprise was that human beings live on the face of the earth." In other words, this meant that Mr. Rawlinson had ceased to entertain the sweeping and promiscuous opinions held by the late General Board of Health and by the present Army Sanitary Commission on the injurious influence of "impure water" on health, and of which he was long so energetic a promoter, and which he, by a trip of the memory, assigned to chemists and medical officers of health of the present time, and has not seen his way to accept the more precise and limited application of "impure water" in its relation to disease as set forth by such authorities as Dr. Parkes, Dr. Gairdner, and others.

THE MEDICAL BENEVOLENT COLLEGE.

THE committee of the governors of this institution, formed some time ago for the purpose of improving the mode of election of beneficiaries of the College, have issued a document, signed by their chairman, Dr. Charles J. Hare, stating that as to the two suggestions put forward by the committee (and copied at the time of their distribution in THE LANCET), assenting replies were returned by 2728 governors, 348 preferring the existing plan. There is, therefore, a considerable *majority of the total number* of governors in favour of the views held by the committee (and, we may add, by THE LANCET), and a majority of more than *seven to one* of those who have troubled themselves to reply. These statistics speak for themselves, and need no comment beyond the natural one that the Council of the College will now, as a representative body, take an early opportunity of adopting the views of those by whom this institution is maintained in its present state of usefulness.

A special meeting of the Election Reform Committee instituted by certain governors of the Medical Benevolent College was held on Tuesday, at which it was agreed to send a circular letter to all those who objected to the present system, urging their attendance at the annual meeting of governors of the College, which will be held in Soho-square on the 29th instant, in order to support the change recommended by the Reform Committee.

THE SLUMS OF LONDON.

WE desire to direct the attention of our readers to a well-written article on this subject that appears in *Macmillan* of the current month, from the pen of Mr. McCullagh Torrens, M.P. It reviews very ably the provisions of the respective Acts that became law in 1868 and 1875, and shows how very little has as yet been done to relieve the unhealthy and miserable condition of the working classes, caused almost exclusively by overcrowding. The Act of 1868 was utterly shorn of all its usefulness in the House of Lords, where the compensation and rebuilding clauses were struck out. It was perceived at once that local authorities would not be disposed to demolish unless they were empowered by law to rebuild,

and that the old pestilential and overcrowded slums would still continue to exist, affording lucrative rents to the landlords. And so, ten years' experience having assured all interested of its inutility in its present mutilated form, the Bill as it went through the Commons in 1868 has been re-introduced, and read a second time.

A critical examination of Mr. Cross's Act shows that up to the present time it has been eminently unsuccessful. Mr. Torrens reminds his readers of the chief schemes of demolition that have been sanctioned, but not yet carried out, notably those in the districts of Bloomsbury, Islington, and Whitechapel. For, as the writer says, "the Act of 1875 stopped short at the point of rebuilding by the local authority where private enterprise failed to do so." In point of fact, the Legislature, in trusting to the latter, leaned upon a bruised reed. So that, as a practical consequence, great spaces have been laid waste, and we have up to the present time waited in vain for any signs of reconstruction.

MEDICAL STUDENTS AND THE SELECT COMMITTEE.

UNDER the Medical Acts and the Regulations of the General Medical Council, medical students have a distinct medical status. They are registered by the medical registrars, and pay fees for registration. They have undoubtedly a deep interest in the impending investigation into the existing system of medical examinations and the working of the General Medical Council. They have a strong case to make out. They can truly say that the Medical Council has done much against them, and nothing for them. It has done much to add to the severity of examinations, and nothing to add to efficiency of education. They have a clear right to ask that if the examinations of the future are to go on increasing in severity they shall be consolidated. "The College and Hall" must no longer give a double worry for a single purpose. The want of the State is not apothecaries and surgeons so much as good general practitioners. They have, further, the right to be heard in asking that the Medical Council shall be so recast as to be made in reality what it is in name—a Council of Medical Education.

NAVAL NOTES.

THE *United Service Gazette* follows our example at last, in calling attention to the vast amount of discomfort, sickness, and temporary paralysis caused in the Government dockyards by lead-poisoning. Several preparations are now being tried on the double bottom of H.M.S. *Devastation*, which may possibly obviate the use of all preparations containing lead; if so, the order to discontinue it cannot be given too soon.

Drunkenness seems to exist to a great extent in the naval brigades as well as the army now campaigning in and around Zululand. We are informed by a correspondent who has lately been travelling in South Africa, that the primary cause of this unsatisfactory state of things is not so much the quantity as the quality of the liquor consumed. A very inferior variety of Schiedam, called in Africa "square gin," is much sold; but the cheapest and worst drink used by the lower classes is a vile product of potato and other vegetable spirit, called "cango," a compound worse than arrack in its effects on the stomach and brain. No laws can be too stringent to prevent its sale in the neighbourhood of our sailors and soldiers, for its effects are demoralising in the extreme.

His Royal Highness the Prince of Wales has signified his intention of distributing the prizes at the Royal Naval School at Greenwich next month. Nearly a thousand sons of seamen and marines of the fleet are now being educated at the school, the sanitary condition of which, under the medical surveillance of Mr. Armstrong, has always been excellent.

H.M.SS. *Boadicea* and *Flora* are at last free from small-pox, and the colonial authorities had, by last advices, released them and their crews from quarantine.

It is to be hoped that Captain Markham, in the preparations for his summer Arctic expedition, has not forgotten certain antiscorbutic items of outfit, the misuse (or rather non-use) of which caused such disastrous results in the last Admiralty expedition.

QUEEN CHARLOTTE'S LYING-IN HOSPITAL.

THE Registrar-General reports in his last weekly return that the Queen Charlotte's Lying-in Hospital is now closed. It appears that during the week ending last Saturday the death of another woman from puerperal fever, and the deaths of two more infants, of which one was certified from erysipelas, were recorded in this institution. Thus, in the six weeks ending the 10th instant, the deaths of eleven mothers and of twenty infants were registered in this lying-in hospital. The General Lying-in Hospital in Lambeth, which was closed in September, 1877, in consequence of epidemic fatality of puerperal fever, has not since been re-opened. We have reason to believe that the City of London Lying-in Hospital has also been closed for some time. In the British Lying-in Hospital 176 deliveries occurred during 1878, and the deaths of two mothers and of eight infants were registered.

THE CORONERS BILL.

WE have received a copy of the memorial compiled by practitioners of Liverpool and presented to the members of that borough, which protests against Clause 30 of the Bill, because, if retained, it will perpetuate the practice of requiring medical officers of public hospitals to give evidence at inquests without fee. The memorialists recite that they have always regarded this practice as unjust, both to the medical officers and the charities, inasmuch as the important services required are distinctly for the public benefit, whereas the time occupied in the performance is time which, but for them, would be devoted to the charities. We take leave to doubt whether the latter reason, as given here, will strengthen the cause much. But let that pass. The cause is, as we have often recorded, a strong one, and good wine needs no bush. The petition appears to be signed by all or nearly all the medical men practising at Liverpool.

A COUNTY COURT SUIT BY A MEMBER OF THE COLLEGE OF PHYSICIANS.

DR. GARSTANG, who is M.D. St. Andrews, and member of the Royal College of Physicians, London, lately, in the Blackburn County Court, sued Mr. William Arthur for the recovery of £30 12s., for attendance for twenty-nine days, from August 27th. There were at parts of the time four patients in all, and two were ill with enteric fever. Dr. Garstang took the ground of a physician. "He never made a bill. The rule was to pay a physician his guinea a visit." On being compared to a neighbouring practitioner, Dr. Morley, he is reported to have said that the defendant had no right to compare him with Dr. Morley; he might as well compare Sir William Jenner with Dr. Morley. Dr. Garstang admitted that he had been a general practitioner, and saw old patients for less than a guinea, but only as a favour. A member of the College of Physicians is of course entitled to act as a physician, and charge accordingly; but the distinction should be made very clear, beyond all risk of doubt, especially in a provincial town. No man should practise in both capacities. A physician in the strict sense of the word is one who sees a patient occasionally in consultation with a general practitioner, and, to e

strict, a physician acting in the spirit and on the letter of the bye-laws does not go into a county court; his fee is an honorarium. We cannot adopt or admire the line of argument by which Dr. Garstang supported his case. He acted, to all intents and purposes, as a general practitioner, and his service was very efficient. Moreover, he attended, not one patient, but three or four, and paid nightly visits besides. His case would have been stronger if the consciousness of membership of the College had been less dominant, and his sense of the value of the services of general practice a little higher. The jury found a verdict for the plaintiff for £20, with costs on the higher scale.

THE SELECT COMMITTEE ON MEDICAL REFORM.

IT is expected that the Select Committee to inquire into the whole subject of medical reform will be nominated on Monday, and may proceed to take evidence soon thereafter. Those who have communications to make to the committee should lose no time in making them after its appointment. We have only to express a hope that as the Government has acted wisely in granting the committee, it will nominate a committee so fair in its composition as to ensure all reasonable parties being heard.

UNIVERSITY COLLEGE.

DR. WALSHE will preside at the distribution of prizes to the students of the Faculty of Medicine, which is fixed to take place on Tuesday, the 20th instant, at 3 P.M. Dr. Walshe is known to the present generation of students and junior practitioners almost solely by his great works on the Heart and Lungs. His powers of oratory, however, are not likely to be forgotten by those who enjoyed his teaching at University College, and many of his former pupils will, we doubt not, be glad of the opportunity which will be afforded them on Tuesday of welcoming their great master to the scene of his former labours.

MEETING OF THE EXECUTIVE COMMITTEE OF THE MEDICAL COUNCIL.

SUMMONSES have been issued for a meeting of the Executive Committee of the General Medical Council on Friday next, the 23rd inst.

We fear it is now too late for the Council or its Executive Committee to do anything to put the Council into harmony with the profession at this juncture. Let us hope that some of its leading members will rise to the occasion, and help those who wish to see the General Medical Council more powerful for good and the medical examining boards more independent.

HOSPITAL SATURDAY IN BIRMINGHAM.

IN spite of the badness of trade and general prognostications of failure, the collections in the factories and workshops of Birmingham last Saturday realised £2481, an amount larger by £238 than was received on Hospital Saturday last year.

The number of contributions was 686, and the amounts ranged from 1*s.* 6*d.* to £112. All donations, irrespective of amount, are acknowledged in the local papers, and too much credit cannot be given to all who have worked to bring about so successful a collection for so excellent a cause.

MELANCHOLY DEATH OF A STUDENT.

WE deeply regret to record the death of Thos. Pemberton Pemberton, the second son of Mr. Oliver Pemberton, surgeon, of Birmingham. The deceased gentleman was a student of high promise at St. Bartholomew's Hospital. It would seem that through stress of brain-work his mind became affected, and he died from injuries inflicted by his own hand while labouring under temporary insanity. Our sympathy with the distinguished member of our profession who has sustained this loss of a son is mingled with deep regret for the occurrence, which is another instance of the effect of overwork provoked by the system of over-examination.

THE LATE DR. MURCHISON.

AT a largely attended meeting of the students and resident medical and surgical staff of St. Thomas's Hospital, held in the Medical Theatre on the 9th inst., the following resolution was passed:—

"That this meeting—wishing to express its sense of the great loss which St. Thomas's Hospital has sustained by the death of Dr. Murchison, and also its appreciation of the great benefits which it has received from his teaching—desires to assist in raising some permanent memorial of him, and is anxious to obtain the co-operation of all old students of this hospital and of all who may be interested in the scheme."

A committee was then elected to confer with the staff as to the most suitable form for such a memorial to take. The committee was empowered to add to its number, and to take such steps as it may deem necessary to make known the objects of the meeting.

AT a special meeting of the Fellows of the College of Physicians in Ireland, held on the 9th inst., it was unanimously resolved that the distinction of honorary fellowship of the College should be conferred on Surgeon-Major Reynolds, in appreciation of his memorable services in the action at Rorke's Drift, Zululand, on Jan. 22nd, 1879. The standing orders relative to the proposing and election of honorary Fellows were suspended in this instance, and the action of the College shows a very marked contrast to the apathy of the military authorities, who appear to have overlooked the gallant conduct of Surgeon-Major Reynolds.

WE are glad to direct the attention of our readers to a very good map of South Africa and Zululand, just published by Mr. Wyld, of Charing-cross. A contributor who has recently returned from an extended tour in South Africa has examined it closely, and reports it as being the best map of the entire district that he has as yet seen. The scale is convenient, the names are very distinct, and there are various historical dates introduced that increase its value considerably.

DEPUTY SURGEON-GENERAL S. C. TOWNSEND, H.M. Indian Medical Service, has been appointed principal medical officer of General Roberts' force in the Kurram Valley, *vice* Deputy Surgeon-General F. Allen, C.B., who has been compelled by ill-health to resign the post.

MR. H. R. SWANZY, lecturer on ophthalmic surgery in the College, Mr. Bennett, and Mr. Baker, will be candidates for a seat on the Council of the Royal College of Surgeons in Ireland at the annual election to be held the first Monday in June.

THE Chelmsford urban and rural sanitary authorities are engaged in considering a joint scheme of sewerage, of which the cost is estimated at £20,000. The scheme includes Chelmsford, Springfield, Great Baddow, and Widford.

THE Duke of Edinburgh will preside at the next dinner of the Edinburgh University Club, which will be held at the Grosvenor Gallery on the 27th inst.

SURGEON-MAJOR WILLIAM JAY, A.M.D., has been ordered to join the field hospital of the Kurram Valley force established on the Peiwar heights.

New Inventions.

VACCINATING CASE AND PORTABLE VACCINE TUBE HOLDER.

DR. WARD COUSINS, of Southsea, who has introduced the above, supplies the following description :—

The Vaccinating Case contains two vaccine tube holders, each forming a receptacle for a number of tubes, one lancet and tube-cutter, and wax tapers for sealing the charged capillary tubes.

The capillary tube is the best lymph-carrier, and the introduction of the holder is intended to promote its universal adoption among practitioners. By this little instrument all the objections to the capillary tube are completely overcome, and its application in practice is rendered extremely easy and convenient.

For charging the capillary tube, take out the glass stopper, and pass the tube to the extent of an inch through the small opening in the india-rubber cap. On inserting the tube in the lymph, that fluid will begin to ascend it; then by gentle mouth action at the open end of the holder it will be drawn into the centre of the tube.

To discharge the lymph, break off the sealed ends of the tube in the "tube-cutter" which is affixed to the lancet, and introduce it into the holder. The required quantity of lymph can be rapidly discharged by gently blowing through the holder, and the remainder can be then replaced in the centre of the tube, and the tube resealed. By this double action the holder enables the vaccinator to utilise one charged tube for two or more operations.

HEALTH OF LARGE ENGLISH TOWNS
IN THE NINETEENTH WEEK OF 1879.

DURING last week 5152 births and 3168 deaths were registered in twenty of the largest English towns. The births were 30 and the deaths 202 below the average weekly numbers during 1878. The deaths showed a further decline of 147 from recent weekly numbers, and the annual rate per 1000, which had declined from 29·1 to 23·4 in the nine preceding weeks, further fell last week to 22·4, a lower rate than has prevailed in these towns since the middle of November last. The lowest rates in these towns last week were 12·9 in Norwich, 15·8 in Brighton, 16·7 in Bradford, and 19·0 in Portsmouth. The rates in the other towns ranged upwards to 24·8 in Manchester, 25·3 in Oldham, 27·1 in Wolverhampton, and 29·5 in Newcastle-upon-Tyne. The high rates in Oldham and Newcastle-upon-Tyne were, in great measure, due to exceptional zymotic fatality. The deaths referred to the seven principal zymotic diseases in the twenty towns last week declined to 367, from 426 and 423 in the two preceding weeks; they included 122 from whooping-cough, 81 from measles, 79 from scarlet fever, and 37 from fever, principally enteric. The annual death-rate from these seven diseases averaged 2·6 per 1000 in the twenty towns; it ranged from 0·5 and 1·0 in Bradford and Brighton to 4·7 and 6·0 in Oldham and Newcastle-upon-Tyne. Whooping-cough showed fatal prevalence in Manchester and Newcastle-upon-Tyne; scarlet fever in Oldham, Newcastle-upon-Tyne, and Hull; and measles in Salford. The deaths referred to fever, principally enteric, showed the largest proportional excess in Portsmouth and Norwich. Small-pox caused 5 and diphtheria 11 deaths in London, whereas no fatal case of small-pox, and only three of diphtheria were returned in the nineteen large provincial towns. The number of small-pox patients in the Metropolitan Asylum Hospitals has steadily declined during the past eleven weeks from 353 to 204 on Saturday last; 33 new cases of small-pox were, however, admitted to these hospitals during last week. The Highgate Small-pox Hospital contained 21 patients on Saturday last, against 11, 12, and 20 at the end of the three previous weeks.

Correspondence.

"Audi alteram partem."

SOCIETY FOR THE RELIEF OF WIDOWS AND ORPHANS OF MEDICAL MEN.

To the Editor of THE LANCET.

SIR,—I beg for a space in your columns to enable me to draw attention to the Society for the Relief of Widows and Orphans of Medical Men. It has reached me that it has been authoritatively stated that the Society has no power under their charter to admit members unless they reside in the county of Middlesex, or within the London district post. As I was one of those who took a part in procuring the charter, and as one of the objects of applying for a charter was to enlarge the sphere of the Society, and to accomplish this the name of the Society was altered from the "Society for the Relief of Widows and Orphans of Medical Men in London and its Vicinity," to "of Medical Men," it is with much surprise I learn that an opinion has got abroad that the restrictions that existed before the charter was granted still remain as to the admission of members. Not presuming to challenge this opinion upon my own authority, I have obtained a legal opinion on the question, which is to the following effect—that "it is competent for the Society to make a new bye-law under which members may be admitted without any reference to their place of residence."

In justice to myself I may add that the alteration I proposed at the annual meeting relative to the holding of the general annual meeting—which was ruled at the meeting as illegal—had been submitted to the legal adviser of the Society, and I was led to understand that he saw no objection to my proposed alteration in the laws.

I now take leave of the question, trusting that others with more influence than I have may be more successful in placing this—I fear still much-needed—Society on a wider basis for usefulness to the profession.

I am, Sir, your obedient servant,

Savile-row, May 13th, 1879. CHARLES HAWKINS.

CARBOLIC ACID AND DEATH AFTER AMPUTATION AT THE HIP-JOINT.

To the Editor of THE LANCET.

SIR,—I have to thank Dr. Glover for giving me the opportunity of attempting to show that my suggestion—it was nothing more—that the thrombosis in the case I recently brought under the notice of the Clinical Society was due to absorption of carbolic acid from the wound is not so "unfounded" as he believes.

I was aware of the facts he relates in his letter; and I would venture to make three remarks upon them :—1. That in death from acute carbolic-acid poisoning asphyxia plays an important part, and may explain the frequency with which a dark-colour and fluidity of the blood have been met with post mortem. 2. That the effects of the slow absorption of a small quantity of the acid from a wound may differ from those caused by a sudden or rapid absorption of a larger quantity from the alimentary canal. 3. That the previous state of the blood in the two classes of cases is often different. For these reasons it seems hardly safe to argue from the one set of cases to the other.

But further, we know that carbolic acid produces intravascular coagulation when applied directly, and for this purpose is made use of in the treatment of nævi; and the following extract from Boehm's article in "Ziemssen," vol. xvii., shows that the records of cases of death by actual

carbolic-acid poisoning do not at all refute my theory: "We can scarcely lay much stress on that quality of the blood on which Husemann insists—viz., that it is very thin and dark-coloured, and coagulates badly—at least, as regards human beings. Apart from the very precarious and doubtful nature of the symptom itself, it does not appear that, in cases of poisoning by carbolic acid in human beings, the quality of the blood has been at all uniform. Indeed, in several of the recorded accounts of post-mortems we find express mention of *extensive clots in the heart and large vessels.*" Nor is it a new thing to find such varying conditions of the blood in disease. In cases of pyæmia, thrombosis is not unfrequent, the blood is often fluid and dark, while in other cases it is firmly coagulated in the heart and large vessels.

In the particular case in point, the fact that all went very well for forty hours after the operation, and that then, suddenly and *coincidently with the excretion of carbolic-acid urine*, the fatal symptoms set in—symptoms which were not merely local and limited to the affected limb, but general,—was certainly sufficiently striking to warrant an inquiry whether the two conditions were not the effects of one common cause.

Dr. Glover expresses a doubt whether the thrombosis was the cause of death. I cannot understand how it should be, but death was not due to shock, nor to simple exhaustion, nor was there visceral disease to account for it. "The grave blood-change, evidenced by the widespread thrombosis," remained as an explanation of the untoward event—the true one, I believe; and I think the burthen of proof that it was not rests upon those who doubt it.

I must not occupy more of your space, but hope that what I have said will lead my friend, Dr. Glover, to acquit me of broaching "doubtful" and "unfounded" theories.

I am, Sir, your obedient servant,

Gower-street, May 13th, 1879. A. PEARCE GOULD.

P.S.—May I remind Dr. Glover that the fortunes of antiseptic surgery are not bound up with those of carbolic acid?

MEMBRANOUS LARYNGITIS FROM EAU DE COLOGNE.

To the Editor of THE LANCET.

SIR,—In your able report of the discussion at the Medical and Chirurgical Society on the "Report of the Croup and Diphtheria Committee" I notice that Dr. George Johnson as much as implies that the lady whose case I sent to the above Committee (the full account of which appears in the Appendix IV. on page 95 of the said Report), fainted "from pain at stool;" therefore she must have been ill, and probably had diphtheria. The lady in question suffers from piles, never enjoys "rude health," and has frequently fainted at stool in the early morning previous to this time when the eau de Cologne entered the trachea. As to the impossibility of any diphtheritic infection being present to account for the formation of the tracheal cast, over and above the remarks in my report of the case I would beg to refer to Mr. Leggatt, of Eastry, her medical attendant, with whom I saw the case in consultation.

I beg to remain yours faithfully,

T. WHITEHEAD REID,

May 14th, 1879. Surgeon to the Kent and Canterbury Hospital.

DR. DOYLE GLANVILLE.

To the Editor of THE LANCET.

SIR,—In your leader of last Saturday on "Medical Officers as 'Non-Combatants,'" you have fallen into the same error as the Natal newspapers did with regard to Dr. Doyle Glanville. I am sure you will be glad to be able to correct the mistaken report of his having fallen at the disastrous field of Isandhlwana. He is still alive and well, and resides at Fort Bengough, which is about fifteen miles south from Rorke's Drift, and about twelve from the Buffalo River, the Zulu border.

I may add that in the *Graphic* of 29th March are several spirited sketches by Dr. Glanville. One of them represents a church parade in that fort of the officers and non-commissioned officers of the 2nd Battalion 1st Regiment of the Natal Native Contingent, on the first Sabbath *after the battle*. The remaining sketches were all made after the catastrophe at Isandhlwana.

I trust this note will reassure the minds of Dr. Glanville's friends as to his actual safety.

Yours very truly,

Lesmahagow, N.B., 8th May, 1879. J. B. DALZELL.

PARLIAMENTARY PROCEEDINGS.

HOUSE OF LORDS.

Thursday, May 8th.

HABITUAL DRUNKARDS BILL.

THE EARL of SHAFTESBURY, in moving the second reading of the Habitual Drunkards Bill, which had come up from the Commons, pointed out at some length the evils inflicted on themselves and society at large by those to whom the measure was to apply. His experience as a Commissioner in Lunacy enabled him to say that in many instances lunacy followed habitual drunkenness. Again, the pernicious effects of over-indulgence in strong drink developed themselves in the offspring of drunkards. A strong case was made out for the interference of the Legislature. The incarceration under the Bill would be voluntary. It enacted that persons might be admitted to retreats for habitual drunkards on their own application, which was to be in writing, and be accompanied by the statutory declaration of two persons that the applicant was an habitual drunkard. The interpretation clause described an habitual drunkard within the meaning of the Act as "a person who by reason of habitual intemperate drinking of intoxicating liquors is dangerous to himself or herself or to others or incapable of managing himself or herself and his or her affairs." He had visited a retreat for women in which from seventy to eighty habitual drunkards had been cured with the best results; and he anticipated much good from the Bill, and from the retreats which would be licensed under it. These would be periodically inspected, and be under the supervision of the Secretary for the Home Department.

Lord STANLEY of ALDERLEY supported the Bill, but suggested amendments in various clauses.

Lord BEAUCHAMP said the powers contained in the Bill were extensive and vague; but they would be carried out under the supervision of the Secretary of State. To apply it as a punishment to persons who had been convicted would be to destroy its character, which was to treat habitual drunkenness as a disease. It placed no burden on the ratepayers, and he thought their lordships would do well to pass it.

Lord SELBORNE observed that perhaps over-caution was the fault of this Bill. It contained so many safeguards that he doubted whether it went far enough. The habitual drunkard was at least as dangerous to society as the lunatic.

Lord ABERDARE pointed out that the Bill would not reach drunkards among the lower orders, who were a very numerous class.

The Bill was read a second time.

HOUSE OF COMMONS.

Thursday, May 8th.

THE ARMY MEDICAL DEPARTMENT.

Dr. WARD asked the Secretary of War whether any recognition had been or was to be made of the services and sacrifices of the members of the Army Medical Department engaged in the campaign in South Africa.

Colonel STANLEY.—It is premature to consider what rewards or honours should be given for the present campaign; and this applies to the Army Medical Department, as well as to the combatant branches of the service. There is an exception to this in respect to the defence of Rorke's Drift. In that case Surgeon Reynolds was promoted to be Surgeon-Major, but his promotion, like that of Lieutenants Chard and Bromhead, was a special one.

Monday, May 12th.

MEDICAL REFORM.

In reply to Mr. ERRINGTON, who asked whether the Vice-President of the Council would agree to refer the question of medical reform to a Select Committee,

Lord G. HAMILTON said,—I stated some time back the course that the Government intended to pursue—viz., to embody in a Bill those educational reforms which had met,

after protracted discussion, with the approval of the vast majority of the medical profession, and to refer to a Select Committee the disputed question of the constitution of the Medical Council, undertaking that the Government Bill should not be proceeded with until the report of the committee had been received. This course I have been unable to carry out, as the appointment of the committee is blocked by an amendment of the hon. gentleman, which he will not withdraw. We are, therefore, in this position—that we must either accede to the proposal of the hon. gentleman to refer all the questions contained in all the Medical Bills to a Select Committee or postpone indefinitely—for the same difficulty would arise next session—the prospect of medical reform. As this is a contingency much feared by medical reformers, we are ready to adopt the inconvenient procedure forced on us rather than sacrifice our Bill, and if, therefore, those hon. gentlemen who have amendments to the Medical Bills will withdraw them so as to allow them to be read a second time, I will move that they be referred in their entirety to a Select Committee.

Notices have since been placed on the paper to this effect.

Obituary.

FREDERICK JOHN BROWN, M.D. LOND. & EDIN., F.R.C.S. (EXAM.)

WITH regret we have to record the death of this esteemed member of our profession, which occurred on the 27th of April last, at the age of fifty-five years. He was the fifth son of Dr. Robert Brown, for many years medical superintendent of the quarantine station at Stangate Creek. In 1845 Dr. Brown obtained the London University Medical Scholarship, and was also gold medalist the same year. He entered the navy in 1846, and joined in the struggle going on for the removal of the disadvantages under which the medical department of the service then laboured. In order to carry out his views more fully he left the navy, and commenced practice at Chatham, ultimately removing to Rochester, where he was appointed consulting surgeon to the Chatham St. Bartholomew's Hospital, which post he held till struck down by his last illness. In March of last year he was attacked with pneumonia and hæmoptysis, from which he recovered, and continued work steadily till October. Bleeding with an accession of pneumonia again occurred, from which he suffered severely at intervals during the winter. In February of the current year laryngeal inflammation supervened, to which and the destructive inflammation he succumbed on the above date, beloved and admired by all who knew him. Dr. Brown was a man distinguished for great decision of character. He knew his duty, and he did it with all his might.

Medical News.

ROYAL COLLEGE OF PHYSICIANS OF LONDON.—The following gentlemen were admitted Fellows of the College on May 9th:—

Bagshawe, Frederick, M.D. Camb., St. Leonards.
Edis, Arthur Wellesley, M.D. Lond., Wimpole-street.
Gowers, Wm. Richard, M.D. Lond., Queen Anne-street.
Greenfield, Wm. Smith, M.D. Lond., Palace-road, Lambeth.
Irvine, James Pearson, M.D. Lond., Mansfield-street.
Mackenzie, Stephen, M.D. Aberd., Finsbury-square.
Squire, William, M.D. St. And., Orchard-street.
Stevens, Henry, M.D. Lond., Sutton, Surrey.
Taylor, Frederic, M.D. Lond., St. Thomas-street.
Williams, John, M.D. Lond., Barley-street.

ROYAL COLLEGE OF SURGEONS OF ENGLAND.—The following gentlemen passed the Primary Examination in Anatomy and Physiology at meetings of the Board of Examiners on the 9th, 12th, 13th, and 14th inst.:—

George F. Grace, Bristol; Alfred Orchard, Thomas H. Summerhill, Arthur Orton, Walter R. Awdry, and John L. Thomas, Birmingham; Herbert K. Fuller, Cambridge; Henry M. Baylis, Edinburgh; Ellis Davies, Dublin; John R. Jones, John D. M'Vean, and James D. Boyd, Glasgow; Geo. H. Dawson, William Holdsworth, and Charles B. Burton, Leeds; Frederick E. Row, Frederick E. Hubbard, C. R. O. Garrard, Alexander G. Wildey, Henry A. Fotheby, George S. Pollard, Harold R. Osborne, Henry C. Dixon, Herbert R. Rowell, Frank C. Payne, G. L. L. Lawson, John B. Trapp, E. Apthorp, John J. Faraker, and Charles F. Campe, Guy's Hospital; Alfred F. Whitwell, Theodore H. Waller, Arthur H. Jacob, Edward A. Bewes, Henry Holcroft, Charles R. Walker, Stephen C. Townsend, Robert J. Allan, and Joseph L. Hewer, St. Bartholomew's Hospital; Wallace Petherick and John Cleife, St. George's Hospital; Francis A. Stone, H. L. Roche, Richard P. Griffin, and A. J. M. Armstrong, St. Mary's Hospital; Arthur A. Lipscomb, Edward F. Grun, and Fred. W. Harvey, London Hospital; Daniel E. Coronado, Colombia; Syed Hasan, Bengal; Andrew Houman, Charing-cross Hospital; William H. Davis, Newcastle-on-Tyne; L. W. H. Pegler, Edinburgh; Hugh W. Macnamara, Westminster Hospital; Joseph Neumann, Berlin; J. W. Long, Guy's Hospital.

APOTHECARIES' HALL.—The following gentlemen passed their examination in the Science and Practice of Medicine, and received certificates to practise, on May 8th:—

Brookes, Frederick William, Westminster-bridge-road.
Cannock, Charles Watkyn, Blakeney, Gloucestershire.
Fairley, Samuel Barjona, Aston, Birmingham.
Wheeler, Albert, Glenthorne-road, Hammersmith.
Wickham, Walter, St. Bartholomew's Hospital.

COLLEGE OF PHYSICIANS IN IRELAND.—At the May examinations the following obtained the licences in Medicine and Midwifery:—

MEDICINE.—William Richard Bates, Samuel Fitz Richard Beamish, Wallace Brown Croskery, William Edward Fitz Gerald, William Francis Hefferman, John Crawhall Nichol, Thomas Heazle Parke, Jane Elizabeth Waterston.

MIDWIFERY.—Samuel Fitz Richd. Beamish, Wallace Brown Croskery, Wm. Edward Fitz Gerald, Thomas Heazle Parke, Benjamin Thomas M'Creery, Jane Elizabeth Waterston.

THE Belper Guardians have increased the salary of Mr. Richard G. Allen, medical officer to the workhouse, from £26 to £45 per annum.

MR. HERBERT EVERITT, M.R.C.S. Eng., late of Wells, Norfolk, has been elected a member of the Legislative Assembly of Cape Colony, for the division of Queenstown.

DR. HENRY LOVE has been presented with an illuminated address and a marble timepiece by the Committee of the Dublin Total Abstinence Society in recognition of his services during a period of about seven years, and as a mark of respect on the occasion of his marriage.

BRITISH MUSEUM LECTURES.—As on previous occasions, Dr. Carter Blake's class will meet in the galleries of Natural History and Antiquities, British Museum, on May 27th and 29th, and June 3rd and 5th.

TESTIMONIALS.—At the annual dinner of the Royal College of Veterinary Surgeons, on May 5th, Major-General Sir Frederick Fitzwygram, Bart., presiding, Mr. George Fleming, 2nd Life Guards, was presented with a beautiful and valuable chiming hall clock, and a purse of three hundred sovereigns, from the principal members of his profession in this country, as well as in India, Australia, Canada, and the United States of America, in recognition of his great services to veterinary science, and especially to veterinary literature.—Last week a testimonial was presented by the *employés* in the Royal Arsenal, Woolwich, to Surgeon-Major J. P. Cunningham, A.M.D., who is about to proceed on foreign service, expressive of their regret at his departure, and their sincere appreciation of the ability and kindness which had characterised all his dealings with them during the period he had been in medical charge of the Arsenal.

Medical Appointments.

ALLARD, J. H., L.R.C.P.Ed., M.R.C.S.E., has been appointed Medical Officer for the Tewkesbury District and the Workhouse of the Tewkesbury Union, vice W. Allard, F.R.C.S.E., resigned.

BURNES, H. F., L.K.Q.C.P.I. & L.M., L.F.P.S.G. & L.M., L.A.H.D., has been appointed Acting Surgeon to the 28th Middlesex (London Irish) Rifle Volunteers.

CRITCHETT, G. A., M.R.C.S.E., has been appointed Ophthalmic Surgeon to the Royal Free Hospital.

DAVIES, E., M.D., M.R.C.S.E., has been reappointed a Medical Officer of Health for the Wrexham Rural Sanitary District, at £75 per annum, for three years.

DEYNS, F., M.D., L.R.C.P.L., M.R.C.S.E., has been appointed Certifying Factory Surgeon for the District of Penny Stratford.

DUNLOP, A., L.R.C.P.Ed., L.R.C.S.Ed., has been appointed Junior Resident Medical Officer to the Belfast Royal Hospital, vice Clarke, promoted.

EVANS, J. W., M.R.C.S.E., has been appointed House-Surgeon to the Liverpool Eye and Ear Infirmary, vice Stone, appointed an Honorary Assistant-Surgeon.

FISHER, C. H., M.D., M.R.C.S.E., L.S.A.L., has been appointed Medical Officer of Health for the Milton-next-Sittingbourne Urban Sanitary District, at £60 for one year, vice Ray, deceased.

HAWKINS, B. I., L.R.C.P.Ed., M.R.C.S.E., has been appointed Certifying Factory Surgeon for the District of Woburn.

HAYES, T. C., M.D., M.R.C.P.I., has been appointed Physician for the Diseases of Women to the Royal Free Hospital.

JOHNSON, J., M.R.C.S.E., L.S.A.L., has been appointed Medical Officer for the No. 3 District of the Coventry Union, vice Overton, resigned.

JONES, W., L.R.C.P.I., M.R.C.S.E., has been reappointed a Medical Officer of Health for the Wrexham Rural Sanitary District, at £45 per annum, for three years.

LEAHY, A. W. D., M.R.C.S.E., L.S.A.L., has been appointed Surgical Registrar to the Charing-cross Hospital, vice Colquhoun, resigned.

LOW, A. B., M.B., C.M., has been appointed House-Surgeon to the Sunderland and Bishopwearmouth Infirmary, vice Blumer, resigned.

MCCOY, T. J., L.K.Q.C.P.I. & L.M., L.R.C.S.I., has been appointed Medical Officer, Public Vaccinator, &c., for the Cappagh Dispensary District of the Tipperary Union, at £110 per annum and fees, and £20 per annum as Medical Officer of Health, vice Webb.

MORRIS, C. K., M.R.C.S.E., L.S.A.L., has been appointed Certifying Factory Surgeon for the District of Spalding, vice Stiles.

MORRIS, J. H., M.R.C.S.E., L.S.A.L., has been appointed an Honorary Surgeon to the Salford and Pendleton Royal Hospital and Dispensary, Manchester, vice Bradbury, resigned.

PIZEY, G., M.R.C.S.E., L.S.A.L., has been reappointed Medical Officer of Health for the Clevedon Urban Sanitary District, at £30 for one year.

REID, G., M.B., C.M., has been appointed Medical Officer of Health for the Stafford Rural Sanitary District, at £75 for one year.

RICE, W., L.K.Q.C.P.I. & L.M., F.R.C.S.I., has been appointed Resident Medical Superintendent of St. Patrick's Hospital for the Insane, Dublin, vice Lawless, deceased.

SALTER, J. R., M.R.C.S.E., L.S.A.L., has been appointed House-Surgeon to the Kidderminster Infirmary, vice Clubbe, resigned.

SELLS, C. J., L.R.C.P.I., M.R.C.S.E., L.S.A.L., has been appointed Medical Officer for the Worplesdon District of the Guildford Union, vice T. J. Sells, M.R.C.S.E., deceased.

STEEL, J., L.R.C.P.Ed., L.R.C.S.Ed., has been appointed Resident Medical Officer to the Dudley Dispensary, vice Fennell, resigned.

STODDART, W. W., F.C.S., has been appointed Public Analyst for the Borough of Chard for one year.

TOBIN, G., M.R.C.S.E., L.S.A.L., has been reappointed Medical Officer of Health for the Adlington Urban Sanitary District, Lancashire, for one year.

WILLIAMS, J., M.R.C.S.E., L.S.A.L., has been reappointed a Medical Officer of Health for the Holywell Rural Sanitary District, at £35 per annum, for three years.

WILLIAMS, W., M.D., M.R.C.S.E., has been reappointed a Medical Officer of Health for the Holywell Rural Sanitary District, at £35 per annum, for three years.

WRIGHT, A., M.R.C.S.E., L.S.A.L., has been appointed Certifying Factory Surgeon for the District of Hornchurch, Essex, vice Jordison, deceased.

WRIGHT, J., M.R.C.S.E., L.S.A.L., has been appointed Certifying Factory Surgeon for the District of Markfield, Leicestershire.

YATES, J., M.R.C.S.E., L.S.A.L., has been appointed Medical Officer of Health for the Newcastle-under-Lyme Rural Sanitary District, at £40 for one year, vice Warburton, whose appointment has expired.

YOUNG, W., M.D., has been appointed Medical Officer of Health for the Malton Rural Sanitary District, at £100 per annum.

Births, Marriages, and Deaths.

BIRTHS.

TELFER.—On the 2nd ult., at the Royal Naval Hospital, Simon's Town, Cape of Good Hope, the wife of William Telfer, L.R.C.S.Ed., Fleet-Surgeon, of a son.

THOMSON.—On the 6th inst., at Westgate-street, Peterborough, the wife of William Thomson, M.D., of a son.

TURNER.—On the 10th inst., at Ealing, the wife of James Smith Turner, M.R.C.S.E., of a daughter.

WHEELER.—On the 9th inst., at Portland-place, Clapton, the wife of Henry Wheeler, L.R.C.P.I., of a daughter.

WICKSTEED.—On the 10th inst., at Chester House, Weston-super-Mare, the wife of Francis W. S. W. Wicksteed, of a daughter.

MARRIAGES.

GLANVILE—SALMON.—On the 7th inst., at All Saints, Clifton, John Glanville, M.R.C.S.E., to Sarah, daughter of the late John Salmon, Esq.

ROMANO—ULRICH.—On the 22nd March, at Pelotas, Rio Grande do Sul, Brazil, Frederick William Richard Romano, M.D., L.R.C.P.I., &c., to Adelina de Lara Ulrich, youngest daughter of Senhor Antonio Maria Ulrich, of Rio de Janeiro.

DEATHS.

CLARKE.—On the 8th inst., at Dinapore, P. J. Clarke, F.R.C.S.I., Surgeon-Major, Army Medical Department.

CUMMING.—On the 9th inst., at 18, Ainslie-place, Edinburgh, James Cumming, M.D., F.R.C.P.E., aged 32.

HEELIS.—On the 11th inst., at Carshalton, Robert Heelis, M.R.C.S.E., aged 51.

MURRAY.—On the 7th ult., at Ajmere, Rajpootana, Dr. Thomas Murray.

PRICHARD.—On the 26th ult., at Prescott, Baschurch, Salop, Thomas Prichard, M.R.C.S.E., aged 68.

THOMAS.—On the 5th inst., at Highfield, Shepperton, Edmund Fredk. Thomas, L.R.C.P.Ed., aged 31.

N.B.—*A fee of 5s. is charged for the insertion of Notices of Births, Marriages, and Deaths.*

METEOROLOGICAL READINGS.

(Taken daily at 8 a.m. by Steward's Instruments.)

THE LANCET OFFICE, May 15th, 1879.

Date.	Barometer reduced to Sea Level, and 32° F.	Direction of Wind.	Wet Bulb	Dry Bulb	Solar Radia in Vacuo	Max. Temp Shade	Min. Temp	Rain fall.	Remarks at 8.30 A.M.
May 9	29·73	WNW	41	43	73	55	40	..	Overcast
" 10	30·06	N.E.	37	41	89	51	33	..	Cloudy
" 11	30·04	N.W.	42	46	61	51	38	..	Overcast
" 12	30·13	N.W.	49	51	97	64	44	0·09	Cloudy
" 13	30·13	W.	51	56	92	60	44	..	Cloudy
" 14	29·85	S.W.	49	54	93	58	48	..	Cloudy
" 15	29·99	N.W.	44	50	70	50	43	0·17	Cloudy

Notes, Short Comments, and Answers to Correspondents.

TREATMENT OF LUPUS BY ERASION.

DR. W. G. SMITH exhibited a patient at a meeting of the Medical Society of the College of Physicians, Ireland, held last week, whom he had been treating by erasion, and who had suffered from lupus of the face for the past five-and-twenty years. This method of treatment, recommended by Professor Volkmann, of Halle, and afterwards by Mr. Squire, has recently been highly spoken of by Mr. Clement Lucas, who has published a paper on the subject in this journal. Dr. Smith's patient, although only a few weeks had elapsed from the time treatment was commenced, showed much improvement; and, notwithstanding the operation is a painful one, and the haemorrhage considerable, yet the bleeding stops after a short time, and, as Mr. Lucas has pointed out, there is an almost absolute freedom from after-pain, there being no subsequent inflammation, such as is excited by cautery or caustics.

Despairing.—The medical profession recognises no specialists of the class described. The case can be perfectly well treated by any hospital surgeon.

John G.—Foreign degrees do not at present give any legal right of practice. In any Medical Act that may pass, power will be granted to the Medical Council to recognise certain foreign degrees.

DANDRUFF.

To the Editor of THE LANCET.

SIR,—My attention has been called to the inquiry of "M.R.C.S." in your issue of May 3rd, asking for a remedy for a case of dandruff following upon measles. I have recently had under my care a patient suffering from pityriasis capitis in an aggravated form, with alopecia, coming on some weeks after measles. Besides the affection of the scalp, the eruption extended to the face (where it was especially marked in the eyebrows), on the neck, and upper part of the chest. I ordered the following ointment to be applied freely to the affected parts, after washing the scalp every night with Pears's soap:—Acetate of lead, half a scruple; liq. carbonis detergens, twenty minims; oil of roses, three minims; vaseline, one ounce. After using the above remedy for four days the scalp and face were quite smooth, and there was only a slight desquamation of the skin of the neck and chest. This also disappeared in another week. As the patient was anæmic and a good deal debilitated, I prescribed at the same time iron and arsenic internally. In this case, as in the one spoken of by "M.R.C.S.," the patient had always been subject to dandruff—not, however, to such an extent, nor had the pityriasis before gone beyond the scalp. This attack came on nine weeks after the patient became ill with measles, and had existed for about five weeks. I will only add that in some cases, where the hair is short and the scales are very copious, I have found the tincture of soap, composed as follows, very efficacious as a wash:—Green soap, three ounces; rectified spirits of wine, one ounce and a half; with spirits of lavender, two drachms. Apply this every morning with a sponge, and then wash the scalp thoroughly with warm water; afterwards rub in the ointment.

Yours faithfully,

W. M. WHISTLER, M.D.

Wimpole-street, Cavendish-square, May, 1879.

To the Editor of THE LANCET.

SIR,—In answer to your correspondent's letter as to the best treatment of dandruff of the scalp, the words of the great Abernethy, "to cleanse, dry, and anoint," apply most suitably to these cases. Cleanse the head about every second day with a wash of yelk of egg, dry thoroughly, and use an astringent lotion of borate of soda and glycerine (two drachms of the borate to six ounces of water), and an ointment o cresote every night at bedtime. This treatment in all ordinary and non-congenital cases of pityriasis capitis I find answer very well.

I am, Sir, yours &c.,

JAMES STARTIN,

Surgeon to St. John's Hospital for Skin Diseases.

Sackville-street, W., May 7th, 1879.

ADVERTISING DOCTORS AND QUACK ADVERTISEMENTS.

OUR attention is again directed to advertisements in newspapers, handbills, &c., proceeding from medical men, to the great scandal of their professional friends and neighbours. Better things might be expected of respectable periodicals than the insertion of such advertisements. The press of no sect seems above the temptation to publish advertisements which seem to imply a defect of intelligence on the part of their readers. We have heard readers strongly object to this feature of the religious journals, which entices them into disquisitions on dyspepsia, constipation, the connexion between a good complexion and elder flowers, &c. &c., at times when they are trying to forget the miserable sensations of disease, and the more miserable creatures who seek to thrive upon the credulity of those who have them.

J. M.—Such a certificate could be of little or no value. A death so certified should be entered as not certified.

L.R.C.P. should write to the Office of the Medical Director of the Navy.

ARMY MEDICAL DEPARTMENT: SURGEON-MAJOR REYNOLDS.

To the Editor of THE LANCET.

SIR,—Your excellent *exposé* in your issue of the 3rd instant of the treatment to be expected by those who have any idea of recruiting the shadowy ranks of the A.M.D. at the forthcoming examination to rescue it from its present deplorable condition, by reason of the dearth of candidates, and thus to relieve the authorities of some of the effects of the just retribution which their cheeseparing, muddling, systematic faithlessness and unfairness to the medical officers have brought upon them; together with the significant warning involved in the conspicuous neglect of Lord Chelmsford and Sir Bartle Frere to make any direct reference in their despatches to the heroism of Surgeon-Major Reynolds at Rorke's Drift, whose name and gallant behaviour one cannot but think must have reached their ears with the other details; and, further, the semi-postscripted terms—just falling short of silent contempt—in which the bravery of those other medical and truly combatant officers now risking their lives in an ungrateful service at the Cape is, when mentioned at all, referred to in the despatches (witness Col. Wood's latest), are well calculated to cause intending candidates to pause ere they decide upon the army as a career; and I, for one, who meant to offer myself for a place, have now determined to avoid the A.M.D. But I am by no means solitary in this respect. In this large school the matter is freely discussed, passing events are carefully noted, and any newly-fledged member of the profession here, who moots his intention of having a "shot at the army," is soon dissuaded from the idea when he learns what the true state of affairs is, by consulting his *confrères* or the files of THE LANCET, or even perusing the despatches of our South African commanders in the newspapers.

The A.M.D. has hitherto had but very limited attractions for Guy's students. There are some among us, however, who hoped for better days, and that the new Warrant might inaugurate a new conduct of affairs. This remains to be seen; but, judging from contemporary events, the attitude of the authorities towards medical officers does not seem one which has improved.

It has been announced in the daily papers that the Victoria Cross—the soldier's greatest prize—has been most deservedly conferred on Majors Chard and Bromhead, and those intrepid fellows of the 24th who distinguished themselves at Rorke's Drift. But why, we are all asking, in the name of common fairness, has the honour been withheld from Surgeon-Major Reynolds? True, he has been made a surgeon-major. But to whom does he owe even this? Not to the official despatch-writers. Indeed, were it not for the disinterested admiration of an eye-witness, and your promptness, Sir, in publishing the details of his valour—for it was from THE LANCET that the daily papers copied their account of him,—the authorities would probably never have heard the name of the man who at Rorke's Drift did double duty as surgeon and soldier: now attending the wounded and dying, now, rifle in hand, encouraging the men, and "blazing away" at the enemy. And this is not considered worthy of the Victoria Cross! How, I ask, is this? Is his profession a disqualification? For one cannot but think, had he been a private soldier, he would have been one of the first recommended for the distinction. And as for the promotion he has received, I find he entered the army in 1868. I apprehend, therefore, he would soon have been made surgeon-major in any case, so that the reward that has been bestowed upon him goes for little, especially when compared with the priceless Victoria Cross, which surely he too fully earned on that awful night at Rorke's Drift.

The Secretary for War and the Committee of Inquiry may "cudgel their brains no more" about unification or otherwise; for while they shut their eyes to the facts contained in your article, and the manner in which the distinguished services of medical officers now at the front are being slurred over in the despatches, and miserably rewarded at home, they will, I venture to predict, attract but few to support the ranks of a tottering department—at least from Guy's.

In conclusion, I have only to add that I am no friend of Dr. Reynolds, nor have I even the honour of his acquaintance; but I am one of many would-be candidates who turn away in disgust from a career in which, in addition to the other disadvantages, a few of the more glaring of which you depict, heroic conduct such as that displayed by Surgeon-Major Reynolds at Rorke's Drift goes officially unnoticed and comparatively unrewarded.

Your obedient servant,

Guy's Hospital, May 5th, 1879. P. M. J.

EFFICACY OF REVACCINATION.

IN the Report for 1878 of the Dublin Metropolitan Police, the medical officer draws attention to the efficacy of revaccination as a means of preventing small-pox. He states that during the epidemic of 1871-72, 148 constables were revaccinated, 154 in 1878, and in the first quarter of the present year 129, making a total of 431, of whom not a single member was attacked with the disease. We may add that during 1878, 31 policemen contracted small-pox, which proved fatal in two instances. The operation, Dr. Nedley remarks, has been objected to in consequence of the unreasonable dread of the local inflammation and the constitutional fever which accompany it; but the 154 constables revaccinated last year were on an average one and a half days off duty, notwithstanding the arduous nature of a policeman's duty, and the weight and tightness of his uniform.

Mr. Henry Gibbons.—Another substitute must be provided and approved.

THE THERAPEUTIC TREATMENT OF SEA-SICKNESS.

To the Editor of THE LANCET.

SIR,—I have read with much interest the letter of Mr. Dingle, recommending the inhalation of nitrite of amyl in sea-sickness. Although I have made three voyages as a surgeon in the mercantile marine, the medicine-chests of the steamers in which I have crossed the ocean never contained this drug; so I have not had an opportunity of trying the inhalation. There are, however, a few other drugs of which I should like to record my experience. Constipation is generally associated with sea-sickness. A smart purgative is therefore indicated at the outset. If the nausea persist, after the bowels have been freely relieved, ammonia is a medicine to be avoided. I have found in most instances in which I have administered this drug that its tendency is to aggravate the symptoms. Alcohol in many cases checks the symptoms for a while, and on a Channel voyage may be taken with great benefit. As, however, its effects are not permanent, in long voyages wine should be avoided. In my experience by far the most valuable remedy is the compound tincture of gentian, one drachm in twelve drachms of water, to be repeated occasionally if necessary. In chronic cases ten drops of tincture of ginger, given in effervescence with a little bicarbonate of soda and tartaric acid, is an excellent stimulant to the mucous membrane of the stomach.

Yours obediently,

GLYNN WHITTLE, M.A. Cantab., &c.

École de Médecine, Paris, May 8th, 1879.

To the Editor of THE LANCET.

SIR,—In reply to Mr. Wood's letter in your last issue, I beg to say I meant the nitrite of amyl ($C_5H_{11}NO_2$), and not nitrate, as stated in my former communication. The letter was written at sea, and I had no books in which the drug was mentioned; therefore I simply copied the printed label on the bottle, although I felt very doubtful about its being correct. Had I trusted to my own judgment, the error would not have occurred.

I am, Sir, yours truly,

S.S. *Mirzapore*, May 12th, 1879. W. A. DINGLE, L.R.C.P. Lond.

*** In Mr. Wood's letter inserted last week an error occurred in the formula of the nitrite. It should have been printed as in the note above.—ED. L.

ADULTERATION OF BREAD.

FOR the adulteration of a 4 lb. loaf with sixteen grains of alum, a baker at Derby was fined 10*s*. Considering how often such adulteration must escape detection, was the fine sufficiently severe to prevent repetition of the offence?

Mr. Bracker, (New York.)—The apparatus shall be noticed when forwarded.

SANITARY STATE OF PENZANCE.

To the Editor of THE LANCET.

SIR,—The particulars in my letter to THE LANCET concerning the sanitary condition of Penzance were furnished by a lady, whose son-in-law and daughter were down with fever at the time. These particulars were then believed by me to be strictly true; but I now find that I misunderstood my informant with regard to two of them. Instead of saying that there were 50 cases then under treatment, I should have said that there had been 50 cases since the outbreak of the epidemic. The statement that there had been 9 deaths in one family circle, I now learn was founded on a newspaper paragraph. That the town was not in a good sanitary state may be taken for granted, as 49 cases of typhoid are admitted to have occurred. With respect to the alleged "apathy" of the authorities, there seemed to be every justification for that charge, as at the time my letter was written nothing had been done to remedy the supposed cause of the fever.

I must express my great regret that I should have stated anything which was not strictly true; but at the same time I cannot but think that I was justified in calling public attention to the fact that typhoid fever existed in the town.

I may state that my letter was written at the urgent entreaty of the lady in question, who was naturally much alarmed at the critical condition of her children, and fearful lest others of the household should be laid down, entailing, as that event would have done, increased anxiety, and a ruinous expense to people in their position.

I am, Sir, yours faithfully,

Blackhill, May 13th, 1879. GEO. RENTON.

*** We insert the above in justice to the question raised.—ED. L.

THE EXTENSION OF THE CHEAP SYSTEM OF MEDICINE.

WE are not insensible to the desirableness of cheapening medical advice to the poor, so as to give them no excuse for repairing to herbalists and druggists. But there is a line below which the pretension to prescribe and dispense becomes essentially undignified. We are sorry to say that the line is passed by many practitioners, who must be left to their own sense of what they owe to themselves, to their patients, and to the profession. The systematic payment of small sums is a very different matter, and worthy of all encouragement; but to pretend to sell medical opinion and medicine for sixpence or a shilling is, we repeat, something impracticable.

Beta.—Carbazotic acid is one of the names of picric or trinitrophenic acid. It is produced, according to Watts: (1) By the action of nitric acid on phenic, tribromophenic, and dinitrophenic acids; also on saligenin, salicylous acid, salicin, salicylic acid, nitrosalicylic acid, phlorizin, extract of willow-bark, indigo, conmarin, aloes, benzoin, the resin of Xanthorrœa hastilis, resin of pure balsam, and silk. (2) By boiling trinitranisol with potash ley. Carbazotic acid is used for dyeing silk and wool, especially the former, of a yellow colour.

Surgeon-Major Macpherson's interesting case shall appear in an early number.

"THE ELY GRAMMAR SCHOOL."

To the Editor of THE LANCET.

SIR,—My attention, as clerk to the City of Ely Urban Sanitary Authority, has been directed to a paragraph in THE LANCET of the 26th ult., stating that "Notice had been served on the Governors of the Ely Grammar School, requiring them within seven days to provide sufficient waterclosets, to make a drain and sewer in Upper Hill-street, and to obtain a proper supply of water." The paragraph goes on then to state that the matter had been "pending for a long time," and that it was "highly discreditable that it should be necessary to employ the full force of the law before proper attention is paid to the sanitary condition of an important educational establishment." The paragraph I find has been copied into one of our local papers, and therein headed "Neglect of Health of Ely."

Now, Sir, let me assure you that from beginning to end the statement is entirely devoid of truth, which leads me to conclude that what is imputed to the Grammar School authorities of this city must (unless the paragraph is a pure invention of some malicious scribe) be intended for some *other* town, in which there is an "Upper Hill-street," there being no street of that name in Ely. I must ask you, then, in justice to the city of Ely and to the Grammar School, to insert this letter in your next impression, and if you should find the statement to have been intended for this town, to give at the same time the authority on which it was made.

I need not say that anything which appears in such an influential periodical as THE LANCET is read by medical men from one end of England to the other, and would be accepted as true by the generality of your readers; and as medical men are most frequently consulted by parents as to the sanitary conditions of public schools to which they think of sending their children, and of the towns in which such schools are situated, such a statement is calculated to inflict great injury not only upon our Grammar School, but also upon the general character of the city for salubrity.

If you will permit me (and I think after having directed public attention to the city of Ely and the Grammar School you ought not to hesitate), I should like to subjoin a statement relating to Ely, which, as clerk to the Board and superintendent registrar for the district, I can vouch for.

The Public Health Act, 1848, was introduced into Ely as early as 1850, and in 1855 the whole city was sewered and supplied with water at a cost of £16,500. Previous to the introduction of the Act, the mortality of Ely had been, on an average of the previous seven years, as high as 25·60; the average annual mortality of the first ten years after the application of the Act (viz., 1851–1861) was 20·20; the average annual mortality of the second ten years (1861–1871), 19·53; and the average annual mortality for the seven years ending December 31st, 1878 (which I have computed), I find to be no more than 18·64.

The statistics of the public health of Ely, therefore, show a *continually decreasing* average mortality, which may be summarised as follows:—

Average annual mortality for the seven years immediately preceding the application of the Public Health Act, 1848, to Ely (1843–50)	25·60
The like for the first ten years afterwards (1851–61)	20·20
The like for the second ten years afterwards (1861–71)..	19·53
The like for the last seven years (1871–78)..	18·64

Your obedient servant,

WM. MARSHALL,

Clerk to the Ely Local Board, and Superintendent Registrar for the District of Ely.

Ely Local Board of Health, Ely, May 7th, 1879.

*** The school to which our remarks referred was not the "Ely Grammar School," more properly so called, but the Isle of Ely Grammar School, which is situated at Wisbeach.—ED. L.

Mr. R. H. Williams.—The report of Mr. Harry Leach on the Pollution of the Thames is sent to the members of the Corporation, and should be asked for at the Town Clerk's Office, Guildhall. Captain Calver's report can probably be obtained by application to the Secretary of the Thames Conservancy, Trinity-square, Tower, E.C.

HOUSE CONSTRUCTION.

DR. ALFRED HILL, medical officer of health for Birmingham, delivered a lecture on this subject at the Great Western Hotel in that town on the 24th ult. The lecture was eminently practical in its details, was illustrated by some excellent diagrams, listened to and appreciated by a large audience, and so was in all respects a success. It would be well if other health officers in the provinces, urban and rural, would follow Dr. Hill's example, and endeavour to popularise house sanitation by showing householders how to look after their drains, traps, &c., and so save much preventable disease in their own families.

Mr. Geo. Levick.—We cannot assume any responsibility with regard to persons advertising in our columns. Nothing is known of these matters in the Editorial Department.

W. M. M.—Consult some physician who has made a special study of the nervous system.

THE DALTON FUND.

To the Editor of THE LANCET.

SIR,—Kindly allow me to acknowledge, through the medium of your columns, the following additional donations, and to solicit further contributions. (*Vide* THE LANCET, Jan. 18th and Feb. 20th):—

J. Maple, Esq.	£1 1 0
The Marquis of Salisbury ..	15 0 0
Captain Toller	1 1 0
The Marquis of Bute	3 0 0

Yours faithfully,

Hampstead, May 10th, 1879. H. COOPER ROSE, M.D.

ANTIQUARIAN DISCOVERIES IN FERMANAGH.

RECENTLY on a line of rail now being formed between Sligo and Enniskillen a tumulus of a primitive type has been discovered. The mound, which is supposed to belong to the stone age, is about 12 ft. high by 125 ft. in diameter, and in it numerous human skeletons have been found; also two stone globes, the larger measuring some nineteen inches in circumference, which are supposed by antiquarians to have been symbols of the sun or moon, the principal objects of Pagan worship.

A SUGGESTION.

To the Editor of THE LANCET.

SIR,—I hear that, amongst other works of recent date, a new edition of Foster's Physiology will be shortly announced. Now, this means for those who wish to have the last edition of this excellent work, the almost yearly purchase of a costly book, the last edition being 1878. May I suggest that, if it is to be more than a reprint, the publishers might issue the important additions and corrections (as they did in the case of the second edition of Sidgwick's Ethics) in the form of a supplement. This would be a great boon to the student of limited means, and presumably a proper publishing speculation. Many of those who have the former edition would gladly buy the Supplement, and thus keep themselves abreast of the most recent advancements.

Yours faithfully,

May, 1879. SUPPLEMENT.

DEATH-RATE OF ST. MATTHEWS, CAMBRIDGE.

THE Vicar of St. Matthews, Cambridge, writes to a local paper that, according to the cemetery register, the death-rate in the low-lying district round the church amounts to 24 per 1000, while for the remainder of the parish the rate is only 11 per 1000. The average death-rate for the town is, according to the medical officer's report, 19·5 per 1000, and thus we have tolerably conclusive evidence of special sanitary defect in one particular district. Perhaps the church itself, or rather the cemetery by which it is probably surrounded, may be the cause of the mischief.

Mr. William Parry.—Address the inquiry directly to the Secretary of the Medical Benevolent Fund.

QUEEN CHARLOTTE'S LYING-IN HOSPITAL.

To the Editor of THE LANCET.

SIR,—In reply to the paragraph in your last week's number respecting Queen Charlotte's Hospital, I am requested to inform you that the action you propose, of not admitting patients into the hospital until all danger of infection is passed, was put in force by the Visitors, under the advice of the physicians, a fortnight since, all in-patients' letters being otherwise provided for.—Yours faithfully,

W. C. GRIGG, M.D.,

Physician to Queen Charlotte's Lying-in Hospital.

Curzon-street, W., May 12th, 1879.

Mr. Smith.—The annual election of Fellows into the Council of the College of Surgeons took place last year on the 4th of July. The following were the unsuccessful candidates, with the number of votes polled by each:—Mr. J. Wood, 81; Mr. H. Power, 78; Mr. J. Lister, 76; Mr. T. Bryant, 62; Sir H. Thompson, 58; Mr. H. Lee, 38; and Mr. B. W. Holt, 37. On this occasion 307 Fellows voted, and four papers were declared invalid. Mr. John Simon, the President, took the chair.